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HISTORY AND ROOT

OF THE PRINCIPLE OF THE

CONSERVATION OF ENERGY

BY

ERNST MACH

TRANSLATED FROM THE GERMAN AND ANNOTATED BY

PHILIP E. B. JOURDAIN, M.A. (Cantab.)

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TRANSLATOR'S PREFACE

The pamphlet of fifty-eight pages entitled *Die Geschichte und die Wurzel des Satzes von der Erhaltung der Arbeit.*[1] *Vortrag gehalten in der k. böhm. Gesellschaft der Wissenschaften am 15. Nov. 1871 von E. Mach, Professor der Physik an der Universität Prag* was published at Prague in 1872, and a second—unaltered—edition at Leipzig (Barth) in 1909. To this second edition (pp. iv, 60) were added a short preface and a few notes by Mach himself. This preface is translated below.

Quite apart from the interest which must attach to the first sketch of a way of regarding science which has become of such great importance to students both of science and of the theory of knowledge, this pamphlet is quite essential to the thorough understanding of Mach's work. In the first place, it contains a reprint of Mach's article (1868) on the definition of mass, which is, perhaps, his most important contribution to mechanics; and, in the second place, the discussion of the logical root of the principle of the conservation of energy is fuller than that in any of his later publications.[2]

[1] In the title of this translation, *Arbeit* is translated by *Energy*, as this word conveys a better idea, at the present time, than the older and more literal equivalent of *Work*. In the text, on the other hand, the word *Work* will always be used, as it corresponds more closely to the terminology of science at the time of the first publication of this essay.

[2] Thus, the questions connected with the uniqueness of determination of events are discussed and illustrated very fully in this essay,

It is proper here to give some references to discussions of Mach's point of view in science.

A fairly good general account of Mach's various works was given in Harald Höffding's lectures on modern philosophers held at the University of Copenhagen in 1902;[3] and another account, with a hostile criticism, was given by T. Case in his article "Metaphysics" in the new volumes which make up the tenth edition of the *Encyclopaedia Britannica*.[4] Often valuable criticisms of Mach's position are to be found in the reviews of the first and second editions of the *Analyse der Empfindungen* written by C. Stumpf,[5] Elsas,[6] Lucien Arréat,[7] and W. R. Boyce Gibson.[8]

The last-named writer speaks[9] of the "generous

and it was this essay that formed the starting-point of Petzoldt's development of the view involved.

The essay "On the Principle of the Conservation of Energy" in Mach's *Popular Scientific Lectures* (3d ed., Open Court Publishing Co., 1898, pp. 137–185), though in many respects like the pamphlet of 1872, is not nearly so complete as it is—a remark made by Hans Kleinpeter (*Die Erkenntnistheorie der Naturforschung der Gegenwart*, Leipzig, 1905, p. 150), who therefore pointed out the need for a reprint of this rare pamphlet.

3 In the German translation, by F. Bendixen, of these lectures under the title: *Moderne Philosophen* (Leipzig, 1905), the part relating to Mach is on pp. 104–110. The section devoted to Maxwell, Mach, Hertz, Ostwald, and Avenarius is on pp. 97–127.

4 Vol. XXX, pp. 665–667. Cf. also the references to Mach's work in Ludwig Boltzmann's article "Models" (*ibid.*, pp. 788–790.)

5 *Deutsche Litteraturzeitung*, Nr. 27, 3. Juli, 1886.

6 *Philosophische Monatshefte*, Vol. XXIII, p. 207.

7 *Revue Philosophique*, 1887, p. 80.

8 *Mind*, N.S., Vol. X, pp. 246–264 (No. 38, April, 1901).

9 *Ibid.*, p. 253.

recognition he [Mach] is always ready to give to anyone who succeeds in improving upon his own attempts," and "his still more eager readiness to put fact before theory. With this eagerness to find out the truth is associated a corresponding ardour in developing and applying it when found."

But philosophers seem hardly to have done justice to Mach's work. Mach himself, indeed, has repeatedly disclaimed for himself the name of philosopher; yet, in a sense, any man who forms a general position from which to regard, say, science, is a philosopher.[10] It must be acknowledged that the least satisfactory parts of Mach's writings are those in which he discusses mathematical conceptions, such as numbers and the continuum; and in which he implies that logic is to be founded on a psychological basis; but such things are unconnected with the greater part of his valuable work.

There are three sets of notes to this translation. The first set, referred to by numerals in the body of the text, consists of the notes added by the author to the

[10] Through a reference in the *Jahrbuch über die Fortschritte der Mathematik* for 1904 (Bd. XXXV, p. 78) I learn that D. Wiktorov has published, in Russian, an exposition of Mach's philosophical views, in the periodical whose name, translated, is *Questions of Philosophy and Psychology*, No. 73 (1904, No. 3), pp. 228–313.

J. Baumann (*Archiv für systematische Philos.*, IV, 1897–1898, Heft 1, October, 1897) gave an account of "Mach's philosophy." Cf. also Hönigswald, *Zur Kritik der Mach'schen Philosophie*, Berlin, 1903; and Mach, *Erkenntnis und Irrtum*, 1906, pp. vii–ix. Adolfo Levi ("Il fenomenismo empiristico," *Riv. di Fil.*, T. I., 1909) analyzed the theories of knowledge of Mill, Avenarius, Mach, and Ostwald.

original edition; the second set consists of those added by the author to the reprint of 1909;[11] and the third set, which contains some account of later work by the author and others on subjects connected with the history and root of the principle of the conservation of work, has been added by the translator. Any other notes by the translator, added for the purpose of giving fuller references, are enclosed in square brackets.

Professor Mach has been most kind in carefully reading my manuscript; and so I trust that not all of the freshness, the force of conviction, and the humour of the original are lost in the present translation.

PHILIP E. B. JOURDAIN

THE MANOR HOUSE
BROADWINDSOR
BEAMINSTER, DORSET
November, 1909

[11] These notes are translated, with the exception of one correcting a misprint in the original edition.

AUTHOR'S PREFACE TO THE SECOND EDITION

In this pamphlet, which appeared in 1872, I made the first attempt to give an adequate exposition of my epistemological standpoint—which is based on a study of the physiology of the senses—with respect to science as a whole, and to express it more clearly in so far as it concerns physics. In it both every *metaphysical* and every one-sided *mechanical* view of physics were kept away, and an arrangement, according to the principle of economy of thought, of facts—of what is ascertained by the senses—was recommended. The investigation of the dependence of phenomena on one another was pointed out as the aim of natural science. The digressions, connected with this, on causality, space, and time, may then have appeared far from the point and hasty; but they were developed in my later writings, and do not, perhaps, lie so far from the science of to-day. Here, too, are to be found the fundamental ideas of the *Mechanik* of 1883,[12] of the *Analyse der Empfindungen* of 1886,[13] which was addressed prin-

[12] [*Die Mechanik in ihrer Entwickelung historisch-kritisch dargestellt*, Leipzig, five editions from 1883 to 1904; English translation by T. J. McCormack under the title *The Science of Mechanics*, Open Court Publishing Co., Chicago, three editions from 1893 to 1907 (the third edition of this is quoted hereafter as *Mechanics*).]

[13] [*Beiträge zur Analyse der Empfindungen*, Jena, 1886; Eng. trans. by C. M. Williams under the title *Contributions to the Analysis of the Sensations*, Open Court Publishing Co., Chicago, 1897. A

cipally to biologists, in the *Wärmelehre* of 1896,[14] and in the *Erkenntnis und Irrtum*—a book which treats at length questions of the epistemology of physics—of 1905.[15]

Certainly it is right that, in response to repeated demands, this work, which was out of print twelve years ago, should appear in an *unaltered* form. I could not have entertained sanguine expectations as to the immediate result of my little work; indeed, many years before, Poggendorff had refused for his *Annalen* my short essay on the definition of mass, which definition is now generally accepted. When Max Planck wrote, fifteen years after I did, on the conservation of energy,[16] he had a remark directed against one of my developments, without which remark one would have supposed that he had not seen my pamphlet at all. But it was a ray of hope for me when Kirchhoff[17] pronounced, in 1874, the problem of mechanics to be the complete and simplest description of motions, and this nearly corre-

second, much enlarged, German edition was published at Jena in 1900 under the title: *Die Analyse der Empfindungen und das Verhältnis des Physischen zum Psychischen;* and a fifth edition appeared in 1906.]

[14] [*Die Principien der Wärmelehre historisch-kritisch entwickelt*, Leipzig, 1896; 2d ed., 1900. The 2d edition is hereafter referred to as *Wärmelehre.*]

[15] [*Erkenntnis und Irrtum. Skizzen zur Psychologie der Forschung*, Leipzig, 1905; 2d ed., 1906.]

[16] [*Das Prinzip der Erhaltung der Energie*, Leipzig, 1887; 2d ed., 1909. The reference to Mach's work of 1872 is on p. 156 of the second edition.]

[17] [*Vorlesungen über mathematische Physik, Bd. I, Mechanik*, Leipzig, 1874; 4th ed., 1897.]

sponded to the economical representation of facts. Helm esteemed the principle of the economy of thought and the tendency of my little treatise towards a general science of energetics. And, finally, though H. Hertz did not give an open expression of his sympathy, yet the utterances in his *Mechanik* of 1894[18] coincide as exactly as is possible with my own,[19] considering that Hertz was a supporter of the mechanical and atomic physics and a follower of Kant. So those whose positions are near to mine are not the worst of men. But since, even at the present time, when I have almost reached the limit of human age, I can count on my fingers those whose standpoint is more or less near to my own—men like Stallo,[20] W. K. Clifford, J Popper, W. Ostwald, K. Pearson,[21] F. Wald, and P. Duhem, not to speak of the younger generation—it is evident that in this connexion we have to do with a very small minority. I cannot, then, share the apprehension that appears to lie behind utterances like that of M. Planck,[22] that

[18] [*Die Prinzipe der Mechanik*, Vol. III of Hertz's *Ges. Werke*, Leipzig, 1894; Eng. trans. by D. E. Jones and J. T. Walley, under the title *The Principles of Mechanics*, London, 1899.]

[19] [On Hertz's mechanics, see Mach, *Mechanics*, pp. 548–555.]

[20] [*The Concepts and Theories of Modern Physics*, 4th ed., London, 1900.]

[21] [*The Grammar of Science*, London, 1892; 2d ed., 1900. The account of the laws of motion in W. K. Clifford's book: *The Common Sense of the Exact Sciences* (London, 1885, 5th ed., 1907), which was completed by Pearson, agrees with Mach's views; but this statement was due, not to Clifford, but to Pearson, whose (see pp viii–ix of the work just mentioned) views were developed independently.]

[22] [*Die Einheit des physicalischen Weltbildes*, Leipzig, 1909, pp. 31–38.]

orthodox physics has need of such a powerful speech in its defence. Rather do I fear that, with or without such speeches, the simple, natural, and indeed inevitable reflections which I have tried to stir up will only come into their rights very late.

"Not every physicist is an epistemologist, and not everyone must or can be one. Special investigation claims a whole man, so also does the theory of knowledge."[23] This must be my answer to the excessively naïve demand of a physicist who was justly celebrated and is now dead, that I should wait with my analysis of the sensations until we knew the paths of the atoms in the brain, from which paths all would easily result. The physicist who thinks under the guidance of a working hypothesis usually corrects his concepts sufficiently by accurate comparison of the theory with observation, and has little occasion to trouble himself with the psychology of knowledge. But whoever wishes to criticize a theory of knowledge or instruct others about it, must know it and have thought it out. I cannot admit that my physicist critics have done this, as I will show without difficulty at the proper place.

E. MACH

VIENNA
May, 1909

[23] *Analyse der Empfindungen*, 5th ed., p. 255.

THE HISTORY AND THE ROOT OF THE PRINCIPLE OF THE CONSERVATION OF ENERGY

I

INTRODUCTION

HE who calls to mind the time when he obtained his first view of the world from his mother's teaching will surely remember how upside-down and strange things then appeared to him. For instance, I recollect the fact that I found great difficulties in two phenomena especially. In the first place, I did not understand how people could like letting themselves be ruled by a king even for a minute. The second difficulty was that which Lessing so deliciously put into an epigram, which may be roughly rendered:

"One thing I've often thought is queer,"
Said Jack to Ted, "the which is
"That wealthy folk upon our sphere,
"Alone possess the riches."*

The many fruitless attempts of my mother to help me over these two problems must have led her to form a very poor opinion of my intelligence.

Everybody will remember similar experiences in his own youth. There are two ways of reconciling oneself with actuality: either one grows accustomed to the puzzles and they trouble one no more, or one learns to

*"Es ist doch sonderbar bestellt,"
Sprach Hänschen Schlau zu Vetter Fritzen,
"Dass nur die Reichen in der Welt
"Das meiste Geld besitzen."

understand them by the help of history and to consider them calmly from that point of view.

Quite analogous difficulties lie in wait for us when we go to school and take up more advanced studies, when propositions which have often cost several thousand years' labour of thought are represented to us as self-evident. Here too there is only one way to enlightenment: historical studies.

The following considerations, which, if I except my reading of Kant and Herbart, have arisen quite independently of the influence of others, are based upon some historical studies. The reason why, in discussion of these thoughts with able colleagues of mine, I could not, as a rule, come to agreement, and why my colleagues always tended to seek the ground of such "strange" views in some confusion of mine, was, without doubt, that historical studies are not so generally cultivated as they should be.[1]

However this may be, these thoughts, which, as the notes and quotations from my earlier writings show, are not of very recent date, but which I have held since the year 1862, were not suited for discussion with my colleagues—I, at least, soon tired of such discussions. With the exception of some short notices written on the occasion of other works and in journals little read by physicists, but which may suffice to prove my independence, I have published nothing about these thoughts.

[1] In fact, I have known only one man, Josef Popper, with whom I could discuss the views exposed here without rousing a horrified opposition. Popper and I, indeed, arrived at similar views on many points of physics independently of one another, which fact I take pleasure in mentioning here.

But now, since some renowned investigators have begun to set foot in this province, perhaps I, too, may bring my small contribution to the classification of the questions with which we are concerned. I must protest at once against this investigation being considered a metaphysical one. We are accustomed to call concepts metaphysical, if we have forgotten how we reached them. One can never lose one's footing, or come into collision with facts, if one always keeps in view the path by which one has come. This pamphlet merely contains straightforward reflections on some facts belonging both to natural science and to history.

Perhaps the following lines will also show the value of the historical method in teaching. Indeed, if from history one learned nothing else than the variability of views, it would be invaluable. Of science, more than anything else, Heraclitus's words are true: "One cannot go up the same stream twice." Attempts to fix the fair moment by means of textbooks have always failed. Let us, then, early get used to the fact that science is unfinished, variable.

Whoever knows only one view or one form of a view does not believe that another has ever stood in its place, or that another will ever succeed it; he neither doubts nor tests. If we extol, as we often do, the value of what is called a classical education, we can hardly maintain seriously that this results from an eight-years' discipline of declining and conjugating. We believe, rather, that it can do us no harm to know the point of view of another eminent nation, so that we can, on occasion, put ourselves in a different position from

that in which we have been brought up. The essence of classical education is historical education.

But if this is correct, we have a much too narrow idea of classical education. Not the Greeks alone concern us, but all the cultured people of the past. Indeed there is, for the investigator of nature, a special classical education which consists in the knowledge of the historical development of his science.

Let us not let go the guiding hand of history. History has made all; history can alter all. Let us expect from history all, but first and foremost, and I hope this of my historical investigation, that it may not be too tedious.

II

ON THE HISTORY OF THE THEOREM OF THE CONSERVATION OF WORK

THE place given to the law of the conservation of energy in modern science is such a prominent one that the question as to its validity, which I will try to answer, obtrudes itself, as it were, of itself. I have allowed myself, in the headline, to call the law that of the conservation of work, because it appeared to me to be a name which is understood by all and prevents wrong ideas. Let us call to mind the considerations, laden with misunderstandings, of the great Faraday on the "law of the conservation of force," and a well-known controversy which was not much poorer in obscurities. One should say "law of the conservation of force" only when one, with J. R. Mayer, calls "force" what Euler called "*effort*" and Poncelet "*travail.*" Of course, one cannot find fault with Mayer, who did not get his concepts from the schools, for using his own peculiar names.

Usually the theorem of the conservation of work is expressed in two forms:

1. $\frac{1}{2}\Sigma mv^2 - \frac{1}{2}\Sigma mv_0^2 = \int \Sigma(Xdx + Ydy + Zdz)$; or

2. It is impossible to create work out of nothing, or to construct a *perpetuum mobile.*

This theorem is usually considered as the flower of the mechanical view of the world, as the highest and most general theorem of natural science, to which the thought of many centuries has led.

I will now try to show:

Firstly, that this theorem, in the second form, is by no means so new as one tends to believe; that, indeed, almost all eminent investigators had a more or less confused idea of it, and since the time of Stevinus and Galileo, it has served as the foundation of the most important extensions of the physical sciences.

Secondly, that this theorem by no means stands and falls with the mechanical view of the world, but that its logical root is incomparably deeper in our mind than that view.

In the first place, as for the first part of my assertion, the proof must be drawn from original sources. Although, now, Lagrange, in his celebrated historical introductions to the sections of the *Mécanique analytique*,[1] repeatedly refers to the development of our theorem, one soon finds, if one takes the trouble to consult the originals themselves, that in his exposition this theorem does not play the part which it played in fact.

Although, now, the following facts, with the exception of a few, coincide with those mentioned by Lagrange, we derive from the important passages, given *in extenso*, another view than that which is found in Lagrange's work.

[1] [The first edition of this work was published at Paris in 1788 (1 vol.) under the title *Méchanique analitique*, the second at Paris, 1811–1813 (2 vols.), the third (ed. J. Bertrand), 1853, and the fourth (*Œuvres*, XI, XII, ed. G. Darboux), 1892.]

Let me emphasize only some points:

Simon Stevinus, in his work *Hypomnemata mathematica*, Tom. IV, *De statica*, of 1605,[2] treats of the equilibrium of bodies on inclined planes.

Over a triangular prism *A B C*, one side of which, *A C*, is horizontal, an endless cord or chain is slung, to which at equal distances apart fourteen balls of equal weight are attached, as represented in cross-section in Fig. 1. Since we can imagine the lower symmetrical part of the cord *A B C* taken away, Stevinus concludes that the four balls on *A B* hold in equilibrium the two balls on *B C*. For if the equilibrium were for a moment disturbed, it could never subsist: the cord would keep moving round forever in the same direction—we should have a perpetual motion. He says:

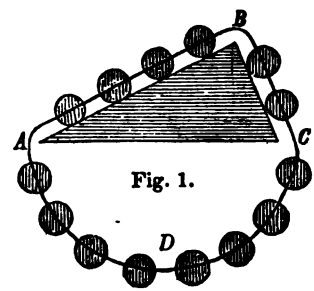


Fig. 1.

> But if this took place, our row or ring of balls would come once more into their original position, and from the same cause the eight globes to the left would again be heavier than the six to the right, and therefore those eight would sink a second time and these six rise, and all the globes would keep up, of themselves, *a continuous and unending motion, which is false.*[3]

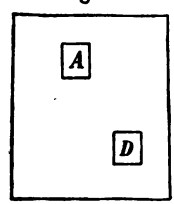
[2] Leiden, 1605, p. 34. [According to Moritz Cantor (*Vorlesungen über Geschichte der Mathematik*, II, 2. Aufl., Leipzig, 1900, p. 572), this work was first published in 1586, and a Latin translation, by Snellius, appeared in 1608. Cf. also Cantor, *ibid.*, pp. 576–577.]

[3] "Atqui hoc si sit, globorum series sive corona eundem situm cum priore habebit, eademque de causa octo globi sinistri pondero-

Stevinus, now, easily derives from this principle the laws of equilibrium on the inclined plane and numerous other fruitful consequences.

In the chapter "Hydrostatics" of the same work, p. 114, Stevinus sets up the following principle: "Aquam datam, datum sibi intra aquam locum servare"—a given mass of water preserves within water its given place. This principle is demonstrated as follows (see Fig. 2):

Fig. 2.



For, assuming it to be possible by natural means, let us suppose that *A* does not preserve the place assigned to it, but sinks down to D. This being posited, the water which succeeds *A* will, for the same reason, also flow down to *D*; *A* will be forced out of its place in *D*; and thus this body of water, for the conditions in it are everywhere the same, *will set up a perpetual motion, which is absurd.*[4]

From this all the principles of hydrostatics are deduced. On this occasion Stevinus also first develops the thought so fruitful for modern analytical mechanics that the equilibrium of a system is not destroyed by the addition of rigid connexions. As we know, the principle of the conservation of the centre of gravity is now sometimes deduced from d'Alembert's principle

siores erunt sex dextris, ideoque rursus octo illi descendent, sex illi ascendent, istique globi ex sese *continuum et aeternum motum efficient, quod est falsum.*"

[4] "A igitur (si ullo modo per naturam fieri possit), locum sibi tributum non servato, ac delabatur in *D*; quibus positis aqua quae ipsi *A* succedit eandem ob causam deffluet in *D*, eademque ab alia istinc expelletur, atque adeo aqua haec (cum ubique eadem ratio sit) *motum instituet perpetuum, quod absurdum fuerit.*"

with the help of that remark. If we were to reproduce Stevinus's demonstration to-day, we should have to change it slightly. We find no difficulty in imagining the cord on the prism possessed of unending uniform motion, if all hindrances are thought away, but we should protest against the assumption of an accelerated motion or even against that of a uniform motion, if the resistances were not removed. Moreover, for greater precision of proof, the string of balls might be replaced by a heavy homogeneous cord of infinite flexibility. But all this does not affect in the least the historical value of Stevinus's thoughts. It is a fact that Stevinus deduces apparently much simpler truths from the principle of an impossible perpetual motion.

In the process of thought which led Galileo to his discoveries at the end of the sixteenth century, the following principle plays an important part: that a body in virtue of the velocity acquired in its descent can rise exactly as high as it fell. This principle, which appears frequently and with much clearness in Galileo's thought, is simply another form of the principle of excluded perpetual motion, as we shall see it is also with Huygens.

Galileo, as we know, arrived at the law of uniformly accelerated motion by *a priori* considerations, as that law which was the "simplest and most natural," after having first assumed a different law which he was compelled to reject. To verify his law he performed experiments with falling bodies on inclined planes, measuring the times of descent by the weights of the water which flowed out of a small orifice in a large

vessel. In this experiment he assumes, as a fundamental principle, that the velocity acquired in descent down an inclined plane always corresponds to the vertical height descended through, a conclusion which for him is the immediate outcome of the fact that a body which has fallen down one inclined plane can, with the velocity it has acquired, rise on another plane of any inclination only to the same vertical height. This principle of the height of ascent also led him, as it seems, to the law of inertia. Let us hear his own masterly words in the *Dialogo terzo* (*Opere*, Padova, 1744, Tom. III). On page 96 we read:

> I take it for granted that the velocities acquired by a body in descent down planes of different inclinations are equal if the heights of those planes are equal.[5]

Then he makes Salviati say in the dialogue:[6]

> What you say seems very probable, but I wish to go farther and by an experiment so to increase the probability of it that it

[5] "Accipio, gradus velocitatis ejusdem mobilis super diversas planorum inclinationes acquisitos tunc esse aequales, cum eorundem planorum elevationes aequales sint."

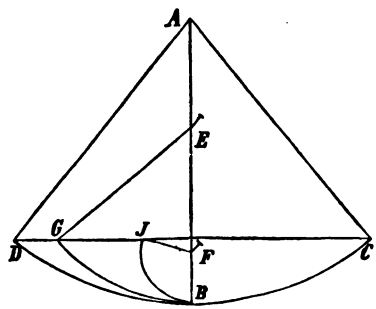
[6] "Voi molto probabilmente discorrete, ma oltre al veri simile voglio con una esperienza crescer tanto la probabilità, che poco gli manchi all'agguagliarsi ad una ben necessaria dimostrazione. Figuratevi questo foglio essere una parete eretta al orizzonte, e da un chiodo fitto in essa pendere una palla di piombo d'un'oncia, o due, sospesa dal sottil filo *A B* lungo due, o tre braccia perpendicolare all'orizzonte, e nella parete segnate una linea orizzontale *D C* segante a squadra il perpendicolo *A B*, il quale sia lontano dalla parete due dita in circa, trasferendo poi il filo *A B* colla palla in *A C*, lasciata essa palla in libertà, la quale primieramente vedrete scendere descrivendo l'arco *C B D*, e di tanto trapassare il termine *B*, che scorrendo per l'arco *B D* sormonterà fino quasi alla segnata parallela *C D*, restando di per vernirvi per piccolissimo intervallo, toltogli il precisamente

shall amount almost to absolute demonstration. Suppose this sheet of paper to be a vertical wall, and from a nail driven in it a ball of lead weighing two or three ounces to hang by a very fine thread *A B* four or five feet long. (Fig. 3.) On the wall mark a horizontal line *D C* perpendicular to the vertical *A B*,

arrivarvi dall'impedimento dell'aria, e del filo. Dal che possiamo veracemente concludere, che l'impeto acquistato nel punto *B* dalla palla nello scendere per l'arco *C B*, fu tanto, che bastò a risospingersi per un simile arco *B D* alla medesima altezza; fatta, e più volte reiterata cotale esperienza, voglio, che fiechiamo nella parete rasente al perpendicolo *A B* un chiodo come in *E*, ovvero in *F*, che sporga in fuori cinque, o sei dita, e questo acciocchè il filo *A C* tornando come prima a riportar la palla *C* per l'arco *C B*, giunta che ella sia in *B*, inoppando il filo nel chiodo *E*, sia costretta a camminare per la circonferenza *B G* descritta intorno al centro *E*, dal che vedremo quello, che potrà far quel medesimo impeto, che dianzi concepizo nel medesimo termine *B*, sospinse l'istesso mobile per l'arco *E D* all'altezza dell'orizzontale *C D*. Ora, Signori, voi vedrete con gusto condursi la palla all'orizzontale nel punto *G*, e l'istesso accadere, l'intoppo si metesse più basso, come in *F*, dove la palla descriverebbe l'arco *B J*, terminando sempre la sua salita precisamente nella linea *C D*, e quando l'intoppe del chiodo fusse tanto basso, che l'avanzo del filo sotto di lui non arivasse all'altezza di *C D* (il che accaderebbe, quando fusse più vicino all punto *B*, che al segamento dell' *A B* coll'orizzontale *C D*), allora il filo cavalcherebbe il chiodo, e segli avolgerebbe intorno. Questa esperienza non lascia luogo di dubitare della verità del supposto: imperocchè essendo li due archi *C B*, *D B* equali e similmento posti, l'acquisto di momento fatto per la scesa nell'arco *C B*, è il medesimo, che il fatto per la scesa dell'arco *D B;* ma il momento acquistato in *B* per l'arco *C B* è potente a risospingere in su il medesimo mobile per l'arco *B D;* adunque anco il momento acquistato nella scesa *D B* è eguale a quello, che sospigne l'istesso mobile pel medesimo arco da *B* in *D*, sicche universalmente ogni momento acquistato per la scesa dun arco è eguale a quello, che può far risalire l'istesso mobile pel medesimo arco: ma i momenti tutti che fanno risalire per tutti gli archi *B D*, *B G*, *B J* sono eguali, poichè son fatti dal istesso medesimo momento acquistato per la scesa *C B*, come mostra l'esperienza: adunque tutti i momenti, che si acquistano per le scese negli archi *D B*, *G B*, *J B* sono eguali."

which latter ought to hang about two inches from the wall. If now the thread *A B* with the ball attached take the position *A C* and the ball be let go, you will see the ball first descend through the arc *C B* and passing beyond *B* rise through the arc *B D* almost to the level of the line *C D*, being prevented from reaching it exactly by the resistance of the air and the thread. From this we may truly conclude that its impetus at the point *B*, acquired by its descent through the arc *C B*, is sufficient to urge it through a similar arc *B D* to the same height. Having performed this experiment and repeated it several times, let us drive in the wall, in the projection of the vertical *A B*, as at *E* or at *F*, a nail five or six inches long, so that the thread *A C*, carrying as before the ball through the arc *C B*, at the moment it reaches the position *A B*, shall strike the nail *E*, and the ball be thus compelled to move up the arc *B G* described about *E* as centre. Then we shall see what the same impetus will here accomplish, acquired now as before at the same point *B*, which then drove the same moving body through the arc *B D* to the height of the horizontal *C D*. Now, gentlemen, you will be pleased to see the ball rise to the horizontal line at the point *G*, and the same thing also happen if the nail be placed lower as at *F*, in which case the ball would describe the arc *B J*, always terminating its ascent precisely at the line *C D*. If the nail be placed so low that the length of thread below it does not reach to the height of *C D* (which would happen if *F* were nearer *B* than to the intersection of *A B* with the horizontal *C D*), then the thread will wind itself about the nail. This experiment leaves no room for doubt as to the truth of the supposition. For as the two arcs *C B*, *D B* are equal and similarly situated, the

Fig. 3.

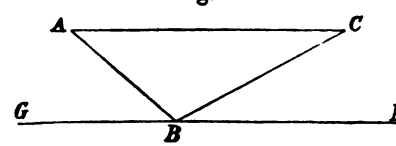


momentum acquired in the descent of the arc CB is the same as that acquired in the descent of the arc DB; but the momentum acquired at B by the descent through the arc CB is capable of driving up the same moving body through the arc BD; hence also the momentum acquired in the descent DB is equal to that which drives the same moving body through the same arc from B to D, so that in general every momentum acquired in the descent of an arc is equal to that which causes the same moving body to ascend through the same arc; but all the momenta which cause the ascent of all the arcs BD, BG, BJ are equal since they are made by the same momentum acquired in the descent CB, as the experiment shows: therefore all the momenta acquired in the descent of the arcs DB, GB, JB are equal.

The remark relative to the pendulum may be applied to the inclined plane and leads to the law of inertia. We read on p. 124:[7]

It is plain now that a movable body, starting from rest at A and descending down the inclined plane AB, acquires a velocity proportional to the increment of its time: the velocity possessed at B is the greatest of the velocities acquired, and by its nature immutably impressed, provided all causes of new acceleration or retardation are taken away: I say acceleration, having in view its possible further progress along the plane extended; retardation, in view of the

Fig. 4.



[7] "Constat jam, quod mobile ex quiete in A descendens per AB, gradus acquirit velocitatis juxta temporis ipsius incrementum: gradum vero in B esse maximum acquisitorum, et suapte natura immutabiliter impressum, sublatis scilicet causis accelerationis novae, aut retardationis: accelerationis inquam, si adhuc super extenso plano ulterius progrederetur; retardationis vero, dum super planum acclive BC fit reflexio: in horizontali autem GH aequabilis motus juxta gradum velocitatis ex A in B acquisitae in infinitum extenderetur.

possibility of its being reversed and made to mount the ascending plane BC. But in the horizontal plane GH its equable motion, according to its velocity as acquired in the descent from A to B, will be continued *ad infinitum*. (Fig. 4.)

Huygens, upon whose shoulders the mantle of Galileo fell, formed a sharper conception of the law of inertia and generalized the principle respecting the heights of ascent which was so fruitful in Galileo's hands. He employed this principle in the solution of the problem of the centre of oscillation and was perfectly clear in the statement that the principle respecting the heights of ascent is identical with the principle of excluded perpetual motion.

The following important passages then occur (Hugenii, *Horologium oscillatorium, pars secunda*). *Hypotheses:*

If gravity did not exist, nor the atmosphere obstruct the motions of bodies, a body would keep up forever the motion once impressed upon it, with equable velocity, in a straight line.[8] [See note 1, p. 75.]

In part four of the *Horologium; De centro oscillationis* we read:

If any number of weights be set in motion by the force of gravity, the common centre of gravity of the weights as a whole cannot possibly rise higher than the place which it occupied when the motion began.

That this hypothesis of ours may arouse no scruples, we will state that it simply means, what no one has ever denied, that heavy bodies do not move *upwards*.—And truly if the devisers of the new machines who make such futile attempts to

[8] "Si gravitas non esset, neque aër motui corporum officeret, unumquodque eorum, acceptum semel motum continuaturum velocitate aequabili, secundum lineam rectam."

construct a perpetual motion would acquaint themselves with this principle, they would easily be brought to see their errors and to understand that the thing is utterly impossible by mechanical means.[9]

There is possibly a Jesuitical mental reservation contained in the words "mechanical means." One might be led to believe from them that Huygens held a non-mechanical perpetual motion as possible.

The generalization of Galileo's principle is still more clearly put in Prop. IV of the same chapter:

If a pendulum, composed of several weights, set in motion from rest, complete any part of its full oscillation, and from that point onwards, the individual weights, with their common connexions dissolved, change their acquired velocities upwards and ascend as far as they can, the common centre of gravity of all will be carried up to the same altitude with that which it occupied before the beginning of the oscillation.[10]

On this last principle, now, which is a generalization, applied to a system of masses (see note 2, p. 80),

[9] "Si pondera quotlibet, vi gravitatis suae, moveri incipiant; non posse centrum gravitatis ex ipsis compositae altius, quam ubi incipiente motu reperiebatur, ascendere.

"Ipsa vero hypothesis nostra quominus scrupulum moveat, nihil aliud sibi velle ostendemus, quam quod nemo unquam negavit, gravia nempe sursum non ferri.—Et sane, si hac eadem uti scirent novorum operum machinatores, qui motum perpetuum irrito conatu moliuntur, facile suos ipsi errores deprehenderent, intelligerentque rem eam mechanica ratione haud quaquam possibilem esse."

[10] "Si pendulum e pluribus ponderibus compositum, atque e quiete dimissum, partem quamcunque oscillationis integrae confecerit, atque inde porro intelligantur pondera ejus singula, relicto communi vinculo, celeritates acquisitas sursum convertere, ac quousque possunt ascendere; hoc facto centrum gravitatis ex omnibus compositae, ad eandem altitudinem reversum erit, quam ante inceptam oscillationem obtinebat."

of one of Galileo's ideas respecting a single mass, and which from Huygens's explanation we recognize as the principle of excluded perpetual motion, Huygens grounds his theory of the centre of oscillation. Lagrange characterizes this principle as precarious and is rejoiced at James Bernoulli's successful attempt, in 1681, to reduce the theory of the centre of oscillation to the laws of the lever, which appeared to him clearer. All the great inquirers of the seventeenth and eighteenth centuries broke a lance on this problem, and it led ultimately, in conjunction with the principle of virtual velocities, to the principle enunciated by d'Alembert in 1743 in his *Traité de dynamique*, though previously employed in a somewhat different form by Euler and Hermann.

Furthermore, the Huygenian principle respecting the heights of ascent became the foundation of the "law of the conservation of living force," as that was enunciated by John and Daniel Bernoulli and employed with such signal success by the latter in his *Hydrodynamics*. The theorems of the Bernoullis differ in form only from Lagrange's expression in the *Analytical Mechanics*.

The manner in which Torricelli reached his famous law of efflux for liquids leads again to our principle. Torricelli assumed that the liquid which flows out of the basal orifice of a vessel cannot by its velocity of efflux ascend to a greater height than its level in the vessel.

Let us next consider a point which belongs to pure mechanics, the history of the principle of *virtual mo-*

tions or virtual velocities. This principle was not first enunciated, as is usually stated, and as Lagrange also asserts, by Galileo, but earlier, by Stevinus. In his *Trochleostatica* of the above-cited work, p. 172, he says:

> Observe that this axiom of statics holds good here:
> As the space of the body acting is to the space of the body acted upon, so is the power of the body acted upon to the power of the body acting.[11]

Galileo, as we know, recognized the truth of the principle in the consideration of the simple machines, and also deduced the laws of the equilibrium of liquids from it.

Torricelli carried the principle back to the properties of the centre of gravity. The condition controlling equilibrium in a simple machine, in which power and load are represented by weights, is that the common centre of gravity of the weights shall not sink. Conversely, if the centre of gravity cannot sink, equilibrium obtains, because heavy bodies of themselves do not move upwards. In this form the principle of virtual velocities is identical with Huygens's principle of the impossibility of a perpetual motion.

John Bernoulli, in 1717, first perceived the universal import of the principle of virtual movements for all systems; a discovery stated in a letter to Varignon. Finally, Lagrange gave a general demonstration of the principle and founded upon it his whole *Analytical Mechanics*. But this general demonstration is based

[11] "Notato autem hic illud staticum axioma etiam locum habere:
"Ut spatium agentis ad spatium patientis
Sic potentia patientis ad potentiam agentis."

after all upon Huygens's and Torricelli's remarks. Lagrange, as is known, conceived simple pulleys arranged in the directions of the forces of the system, passed a cord through these pulleys, and appended to its free extremity a weight which is a common measure of all the forces of the system. With no difficulty, now, the number of elements of each pulley may be so chosen that the forces in question shall be replaced by them. It is then clear that if the weight at the extremity cannot sink, equilibrium subsists, because heavy bodies cannot of themselves move upwards. If we do not go so far, but wish to abide by Torricelli's idea, we may conceive every individual force of the system replaced by a special weight suspended from a cord passing over a pulley in the direction of the force and attached at its point of application. Equilibrium subsists then when the common centre of gravity of all the weights together cannot sink. The fundamental supposition of this demonstration is plainly the impossibility of a perpetual motion.

Lagrange tried in every way to supply a proof free from extraneous elements and fully satisfactory, but without complete success. Nor were his successors more fortunate.

The whole of mechanics is thus based upon an idea, which, though unequivocal, is yet unwonted and not coequal with the other principles and axioms of mechanics. Every student of mechanics, at some stage of his progress, feels the uncomfortableness of this state of affairs; everyone wishes it removed; but seldom is the difficulty stated in words. Accordingly, the

zealous pupil of the science is greatly rejoiced when he reads in a master like Poinsot (*Théorie générale de l'équilibre et du mouvement des systèmes*) the following passage, in which that author is giving his opinion of the *Analytical Mechanics:*

In the meantime, because our attention in that work was first wholly engrossed with the consideration of its beautiful development of mechanics, which seemed to spring complete from a single formula, we naturally believed that the science was completed, and that it only remained to seek the demonstration of the principle of virtual velocities. But that quest brought back all the difficulties that we had overcome by the principle itself. That law so general, wherein are mingled the vague and unfamiliar ideas of infinitely small movements and of perturbations of equilibrium, only grew obscure upon examination; and the work of Lagrange supplying nothing clearer than the march of analysis, we saw plainly that the clouds had appeared lifted from the course of mechanics only because they had, so to speak, been gathered at the very origin of that science.

At bottom, a general demonstration of the principle of virtual velocities would be equivalent to the establishment of the whole of mechanics upon a different basis: for the demonstration of a law which embraces a whole science is neither more nor less than the reduction of that science to another law just as general, but evident, or at least more simple than the first, and which, consequently, would render that useless.[12]

[12] "Cependant, comme dans cet ouvrage on ne fut d'abord attentif qu'à considérer ce beau développement de la mécanique qui semblait sortir tout entière d'une seule et même formule, on crut naturellement que la science était faite, et qu'il ne restait plus qu'à chercher la démonstration du principe des vitesses virtuelles. Mais cette recherche ramena toutes les difficultés qu'on avait franchies par le principe même. Cette loi si générale, où se mêlent des idées vagues et étrangères de mouvements infinement petits et de perturbation d'équilibre, ne fit en quelque sorte que s'obsurcir à l'examen; et le livre de Lagrange n'offrant plus alors rien de clair que la marche des calculs,

According to Poinsot, therefore, a proof of the principle of virtual movements is tantamount to a total rehabilitation of mechanics.

Another circumstance of discomfort to the mathematician is that, in the historical form in which mechanics at present exists, dynamics is founded on statics, whereas it is desirable that in a science which pretends to deductive completeness the more special statical theorems should be deducible from the more general dynamical principles.

In fact, a great master, Gauss, gave expression to this desire in his presentment of the principle of least constraint (Crelle's *Journal für reine und angewandte Mathematik*, Vol. IV, p. 233) in the following words: "Proper as it is that in the gradual development of a science, and in the instruction of individuals, the easy should precede the difficult, the simple the complex, the special the general, yet the mind, when once it has reached a higher point of view, demands the contrary course, in which all statics shall appear simply as a special case of mechanics." Gauss's own principle, now, possesses all the requisites of universality, but its difficulty is that it is not immediately intelligible,

on vit bien que les nuages n'avaient paru levé sur le cours de la mécanique que parcequ'ils étaient, pour ainsi dire, rassemblés à l'origine même de cette science.

"Une démonstration générale du principe des vitesses virtuelles devait au fond revenir a établir le mécanique entière sur une autre base: car la démonstration d'une loi qui embrasse toute une science ne peut être autre chose que la réduction de cette science à une autre loi aussi générale, mais évidente, ou du moins plus simple que la première, et qui partant la rende inutile" (Poinsot, *Éléments de statique*, 10. éd., Paris, 1861, pp. 263–264).

and that Gauss deduced it with the help of d'Alembert's principle, a procedure which left matters where they were before.

Whence, now, is derived this strange part which the principle of virtual motion plays in mechanics? For the present I shall only make this reply. It would be difficult for me to tell the difference of impression which Lagrange's proof of the principle made on me when I first took it up as a student and when I subsequently resumed it after having made historical researches. It first appeared to me insipid, chiefly on account of the pulleys and the cords which did not fit in with the mathematical view, and whose action I would much rather have discovered from the principle itself than have taken for granted. But now that I have studied the history of the science I cannot imagine a more beautiful demonstration.

In fact, through all mechanics it is this selfsame principle of excluded perpetual motion which accomplishes almost everything that displeased Lagrange, but which he still had to employ, at least tacitly, in his own demonstration. If we give this principle its proper place and setting, the paradox is explained.

Let us consider another department of physics, the theory of heat.

S. Carnot, in his *Réflexions sur la puissance motrice du feu*,[13] established the following theorem: Whenever work is performed by means of heat, a certain quantity of heat passes from a warmer to a colder body (supposing that a permanent alteration in the state of the

[13] Paris, 1824. [Cf. a note to p. 38 below.]

acting body does not take place). To the performance of work corresponds a transference of heat. Inversely, with the same amount of the work obtained, one can again transfer the heat from the cooler body to the warmer one. Carnot, now, found that the quantity of heat flowing from the temperature t to the temperature t_1, for a definite performance of work, cannot depend upon the chemical nature of the bodies in question, but only upon these temperatures. If not, a combination of bodies, which would continually generate work out of nothing, could be imagined. Here, then, an important discovery is founded on the principle of excluded perpetual motion. This is without doubt the first extra-mechanical application of the theorem.

Carnot considered the quantity of heat as invariable. Clausius, now, found that with the performance of work, heat not merely flows over from t to t_1, but also a part of it, which is always proportional to the work performed, is lost. By a continued application of the principle of excluded perpetual motion, he found that

$$-\frac{Q}{T}+Q_1\left(\frac{1}{T_1}-\frac{1}{T}\right)=0,$$

where Q denotes the quantity of heat transformed into work and Q_1, that which flows from the absolute temperature T to the absolute temperature T_1.

Special weight has been laid on this vanishing of heat with the performance of work and the formation of heat with the expenditure of mechanical work—which processes were confirmed by the considerations of J. R. Mayer, Helmholtz, and W. Thomson, and by

the experiments of Rumford, Joule, Favre, Silbermann, and many others. From this it was concluded that, if heat can be transformed into mechanical work, heat consists in mechanical processes—in motion. This conclusion, which has spread over the whole cultivated world like wild-fire, had, as an effect, a huge mass of literature on this subject, and now people are everywhere eagerly bent on explaining heat by means of motions; they determine the velocities, the average distances, and the paths of the molecules, and there is hardly a single problem which could not, people say, be completely solved in this way by means of sufficiently long calculations and of different hypotheses. No wonder that in all this clamour the voice of one of the most eminent, that of the great founder of the mechanical theory of heat, J. R. Mayer, is unheard:

> Just as little as, from the connexion between the tendency to fall (*Fallkraft*) and motion, we can ~~conclude that~~ the essence of this tendency is motion, ~~just so little does this~~ conclusion hold for heat. ~~Rather might~~ we conclude the opposite, that, in order to become heat, motion—whether simple or vibrating, like light or radiant heat—must cease to be motion.[14]

We will see later what is the cause of the vanishing of heat with the performance of work.

The second extra-mechanical application of the theorem of excluded perpetual motion was made by Neumann for the analytical foundation of the laws of electrical induction. This is, perhaps, the most talented work of this kind.

[14] *Mechanik der Wärme*, Stuttgart, 1867, p. 9.

Finally, Helmholtz[15] attempted to carry the law of the conservation of work through the whole of physics, and, from this point onwards, the applications of this law to the extension of science are innumerable.

Helmholtz carried the principle through in two ways. We can, said he, set out from the fundamental theorem that work cannot be created out of nothing, and thereby bring physical phenomena into connexion, or we can consider physical processes as molecular processes which are produced by central forces alone—thus by forces which have a potential. For the latter processes, the

[15] [A convenient edition of H. Helmholtz's paper *Ueber die Erhaltung der Kraft* of 1847, together with the notes that Helmholtz himself added to its reprint in his *Wissenschaftliche Abhandlungen* (Vol. I, pp. 12–75), is that in Nr. 1 of Ostwald's *Klassiker der exakten Wissenschaften*. This same series of *Klassiker* also includes, in German translations, and with notes that are often valuable, the following works, which are referred to by Mach in the present work: Galileo's *Discorsi* (notes by Arthur von Oettingen), Nr. 11, 24, and 25; Carnot's work of 1824 (notes by W. Ostwald), Nr. 37; F. E. Neumann's papers on induced electric currents (notes by C. Neumann), Nr. 10 and 36; Clausius's paper of 1850 on thermodynamics (notes by M. Planck), Nr. 99; and Coulomb's papers on the torsion balance (notes by Walter König), Nr. 13. In the same series are some papers of Helmholtz and Kirchhoff on thermodynamics (notes by M. Planck) in Nr. 124 and 101 respectively; and Huygens's *Traité de la lumière* of 1678, in which certain views as to mechanical physics (cf. Mach, *Pop. Sci. Lect.*, 3d ed., Open Court Publishing Co., Chicago, 1898, pp. 155–156) are given, is annotated by E. Lommel and A. von Oettingen in Nr. 20.

We may also add here that Clausius's papers on thermodynamics have been translated into English by W. R. Browne (*The Mechanical Theory of Heat by R. Clausius*, London, 1879; reviewed in *Nature*, February 19, 1880. The German edition was published in Braunschweig, 3 vols., Vol. I, 3d ed., 1887, Vol. II, 2d ed., 1879, Vol. III, 2d ed., 1889–91).]

mechanical law of the conservation of work, in Lagrange's form, of course holds.

As regards the first thought, we must regard it as an important one as containing the generalization of the attempts of Carnot, Mayer, and Neumann to apply the principle outside mechanics. Only we must combat the view, to which Helmholtz inclined, that the principle first came to be accepted through the development of mechanics. In fact, it is older than the whole of mechanics.

This view, now, seems to have been the leading motive in occasioning the second manner of treatment, against which, as I hope to show, very much can be urged.

However this may be, the view that physical phenomena can be reduced to processes of motion and equilibrium of molecules is so universally spread that, at the present time, one can only let people know that one's convictions are opposed to it, with caution, guardedly, and at the risk of rousing the opinion that one is not up to date and has not grasped the trend of modern culture.

To illustrate this point, I will quote a passage from a tract of 1866 on the physical axioms by Wundt,[16] for Wundt is a representative of the modern natural scientific tendency, and his way of thinking is probably that of a great majority of the investigators of natural science. Wundt lays down the following axioms:

1. All causes in nature are motional causes.

[16] *Die physikalischen Axiome und ihre Beziehungen zum Kausalprincip*, Erlangen, 1866.

2. Every motional cause lies outside the object moved.

3. All motional causes act in the direction of the straight line of junction, and so on.

4. The effect of every cause persists.

5. To every effect corresponds an equal counter-effect.

6. Every effect is equivalent to its cause.

Thus, there is no doubt that here all phenomena are thought of as a sum of mechanical events. And, so far as I know, no objection has been raised to Wundt's view. Now, however valuable Wundt's work may be in so far as it relates to mechanics, especially for what concerns the derivation of the axioms, and however much it agrees in that with the thoughts which I have held for many years, I can regard his theorems as mechanical only and not as physical. I will return to this question later.

Thus we have seen, in this historical sketch of many centuries, that our principle of the conservation of work has played a great part as an instrument of research. The second theorem of excluded perpetual motion was always leading to the discovery of mechanical—and later other physical—truths, and can also be considered as the historical foundation of the first theorem. On the other hand, the attempt to regard the whole of physics as mechanics and to make the first theorem the foundation of the second, or to extend the first to the second, is not capable of being misunderstood. Now, this circle is objectionable and rouses one's suspicions. It calls urgently for an investigation.

In the first place it is clear that the principle of excluded perpetual motion cannot be founded on mechanics, since its validity was felt long before the edifice of mechanics was raised. The principle must have another foundation. This view will now be supported if, on closer consideration of the mechanical conception of physics, we find that the latter suffers from being a doubtful anticipation and from one-sidedness, neither of which accusations can be laid against our principle. We will, then, first of all, examine the mechanical view of nature, in order to prove that the said principle is independent of it.

principle of conservation
↑
exclusion of perpetual motion
↑
mechanical concep... of physics

Mach → No

III

MECHANICAL PHYSICS

THE attempt to extend the mechanical theorem of the conservation of work to the theorem of excluded perpetual motion is connected with the rise of the mechanical conceptions of nature, which again was especially stimulated by the progress of the mechanical theory of heat. Let us, then, glance at the theory of heat.

The modern mechanical theory of heat and its view that heat is motion principally rest on the fact that the quantity of heat present decreases in the measure that work is performed and increases in the measure that work is used, provided that this work does not appear in another form. I say the modern theory of heat, for it is well known that the explanation of heat by means of motion had already more than once been given and lost sight of.

If, now, people say, heat vanishes in the measure that it performs work, it cannot be material, and consequently must be motion.

S. Carnot found that whenever heat performs work, a certain quantity of heat goes from a higher temperature-level to a lower one. He supposed in this that the quantity of heat remains constant. A simple analogy is this: If water (say, by means of a water-mill) is to perform work, a certain quantity of it must flow from a

higher to a lower level; the quantity of water remains constant during the process.

When wood swells with dampness, it can perform work, burst open rocks, for example; and some people, as the ancient Egyptians, have used it for that purpose. Now, it would have been easy for an Egyptian wiseacre to have set up a mechanical theory of humidity. If wetness is to do work, it must go from a wetter body to one less wet. Evidently the wiseacre could have added that the quantity of wetness remains constant.

Electricity can perform work when it flows from a body of higher potential to one of lower potential; the quantity of the electricity remains constant.

A body in motion can perform work if it transfers some of its *vis viva* to a body moving more slowly. *Vis viva* can perform work by passing from a higher velocity-level to a lower one; the *vis viva* then decreases.

It would not be difficult to produce such an analogy from every branch of physics. I have intentionally chosen the last, because complete analogy breaks down.

When Clausius brought Carnot's theorem into connexion with the reflexions and experiments of Mayer, Joule, and others, he found that the addition "the quantity of heat remains constant" must be given up. One must, on the other hand, say that a quantity of heat proportional to the work performed vanishes.

"The quantity of water remains constant while work is performed, because it is a substance. The quantity of heat varies because it is not a substance."

These two statements will appear satisfactory to most scientific investigators; and yet both are quite worthless and signify nothing.

We will make this clear by the following question which bright students have sometimes put to me. Is there a mechanical equivalent of electricity as there is a mechanical equivalent of heat? Yes, and no. There is no mechanical equivalent of *quantity* of electricity as there is an equivalent of *quantity* of heat, because the same quantity of electricity has a very different capacity for work, according to the circumstances in which it is ~~placed~~; but ~~there is~~ a mechanical equivalent of electrical energy.

Let us ask another question. Is there a mechanical equivalent of water? No, there is no mechanical equivalent of quantity of water, but there is a mechanical equivalent of weight of water multiplied by its distance of descent.

When a Leyden jar is discharged and work thereby performed, we do not picture to ourselves that the quantity of electricity disappears as work is done, but we simply assume that the electricities come into different positions, equal quantities of positive and negative electricity being united with one another.

What, now, is the reason of this difference of view in our treatment of heat and of electricity? The reason is purely historical, wholly conventional, and, what is still more important, is wholly indifferent. I may be allowed to establish this assertion.

In 1785 Coulomb constructed his torsion balance, by which he was enabled to measure the repulsion of

electrified bodies. Suppose we have two small balls, A, B, which over their whole extent are similarly electrified. These two balls will exert on one another, at a certain distance r of their centres from one another, a certain repulsion p. We bring into contact with B, now, a ball C, suffer both to be equally electrified, and then measure the repulsion of B from A and of C from A at the same distance r. The sum of these repulsions is again p. Accordingly something has remained constant. If we ascribe this effect to a substance, then we infer naturally its constancy. But the essential point of the exposition is the divisibility of the electric force p and not the simile of substance.

In 1838 Riess constructed his electrical air-thermometer (the thermoelectrometer). This gives a measure of the quantity of heat produced by the discharge of jars. This quantity of heat is not proportional tc the quantity of electricity contained in the jar by Coulomb's measure, but if q be this quantity and s be the capacity, is proportional to q^2/s, or, more simply still, to the energy of the charged jar. If, now, we discharge the jar completely through the thermometer, we obtain a certain quantity of heat, W. But if we make the discharge through the thermometer into a second jar, we obtain a quantity less than W. But we may obtain the remainder by completely discharging both jars through the air-thermometer, when it will again be proportional to the energy of the two jars. On the first, incomplete discharge, accordingly, a part of the electricity's capacity for work was lost.

When the charge of a jar produces heat, its energy

is changed and its value by Riess's thermometer is decreased. But by Coulomb's measure the quantity remains unaltered.

Now let us imagine that Riess's thermometer had been invented before Coulomb's torsion balance, which is not a difficult feat of imagination, since both inventions are independent of each other; what would be more natural than that the "quantity" of electricity contained in a jar should be measured by the heat produced in the thermometer? But then, this so-called quantity of electricity would decrease on the production of heat or on the performance of work, whereas it now remains unchanged; in the first case, therefore, electricity would not be a *substance* but a *motion*, whereas now it is still a substance. The reason, therefore, why we have other notions of electricity than we have of heat, is purely historical, accidental, and conventional.

This is also the case with other physical things. Water does not disappear when work is done. Why? Because we measure quantity of water with scales, just as we do electricity. But suppose the capacity of water for work were called quantity, and had to be measured, therefore, by a mill instead of by scales; then this quantity also would disappear as it performed the work. It may, now, be easily conceived that many substances are not so easily got at as water. In that case we should be unable to carry out the one kind of measurement with the scales while many other modes of measurement would still be left us.

In the case of heat, now, the historically established

measure of "quantity" is accidentally the work-value of the heat. Accordingly, its quantity disappears when work is done. But that heat is not a substance follows from this as little as does the opposite conclusion that it is a substance. In Black's case the quantity of heat remains constant because the heat passes into no *other* form of energy.

If anyone to-day should still wish to think of heat as a substance, we might allow that person this liberty with little ado. He would only have to assume that that which we call quantity of heat was the energy of a substance whose quantity remained unaltered, but whose energy changed. In point of fact we might much better say, in analogy with the other terms of physics, energy of heat, instead of quantity of heat.

By means of this reflection, the peculiar character of the second principal theorem of the mechanical theory of heat quite vanishes, and I have shown in another place that we can at once apply it to electrical and other phenomena if we put "potential" instead of "quantity of heat" and "potential function" instead of "absolute temperature." (See note 3, p. 85.)

If, then, we are astonished at the discovery that heat is motion, we are astonished at something which has never been discovered. It is quite irrelevant for scientific purposes whether we think of heat as a substance or not.

If a physicist wished to deceive himself by means of the notation that he himself has chosen—a state of things which cannot be supposed to be—he would behave similarly to many musicians who, after they have long

forgotten how musical notation and softened pitch arose, are actually of the opinion that a piece marked in the key of six flats (G♭) must sound differently from one marked in the key of six sharps (F♯).

If it were not too much for the patience of scientific people, one could easily make good the following statement. Heat is a substance just as much as oxygen is, and it is not a substance just as little as oxygen. Substance is possible phenomenon, a convenient word for a gap in our thoughts.

To us investigators, the concept "soul" is irrelevant and a matter for laughter. But matter is an abstraction of exactly the same kind, just as good and just as bad as it is. We know as much about the soul as we do of matter.

If we explode a mixture of oxygen and hydrogen in an eudiometer-tube, the phenomena of oxygen and hydrogen vanish and are replaced by those of water. We, say, now, that water *consists* of oxygen and hydrogen; but this oxygen and this hydrogen are merely two thoughts or names which, at the sight of water, we keep ready, to describe phenomena which are not present, but which will appear again whenever, as we say, we decompose water.

It is just the same case with oxygen as with latent heat. Both can appear when, at the moment, they cannot yet be remarked. If latent heat is not a substance, oxygen need not be one.

The indestructibility and conservation of matter cannot be urged against me. Let us rather say conservation of *weight;* then we have a pure fact, and we

see at once that it has nothing to do with any theory. This cannot here be carried out any farther.

One thing we maintain, and that is, that in the investigation of nature, we have to deal only with knowledge of the connexion of appearances with one another. What we represent to ourselves behind the appearances exists *only* in our understanding, and has for us only the value of a *memoria technica* or formula, whose form, because it is arbitrary and irrelevant, varies very easily with the standpoint of our culture.

If, now, we merely keep our hold on the new laws as to the connexion between heat and work, it does not matter how we think of heat itself; and similarly in all physics. This way of presentation does not alter the facts in the least. But if this way of presentation is so limited and inflexible that it no longer allows us to follow the many-sidedness of phenomena, it should not be used any more as a formula and will begin to be a hindrance to us in the knowledge of phenomena.

This happens, I think, in the mechanical conception of physics. Let us glance at this conception that all physical phenomena reduce to the equilibrium and movement of molecules and atoms.

According to Wundt, all changes of nature are mere changes of place. All causes are motional causes.[17] Any discussion of the philosophical grounds on which Wundt supports his theory would lead us deep into the speculations of the Eleatics and the Herbartians. Change of place, Wundt holds, is the *only* change of a thing in which a thing remains identical with

[17] *Op. cit.*, p. 26.

itself. If a thing changed *qualitatively*, we should be obliged to imagine that something was annihilated and something else created in its place, which is not to be reconciled with our idea of the identity of the object observed and of the indestructibility of matter. But we have only to remember that the Eleatics encountered difficulties of exactly the same sort in motion. Can we not also imagine that a thing is destroyed in *one* place and in *another* an exactly similar thing created?

It is a bad sign for the mechanical view of the world that it wishes to support itself on such preposterous things, which are thousands of years old. If the ideas of matter, which were made at a lower stage of culture, are not suitable for dealing with the phenomena accessible to those on a higher plane of knowledge, it follows for the true investigator of nature that these ideas must be given up; not that only those phenomena exist, for which ideas that are out of order and have been outlived are suited.

But let us suppose for a moment that all physical events can be reduced to spatial motions of material particles (molecules). What can we do with that supposition? Thereby we suppose that things which can never be seen or touched and only exist in our imagination and understanding, can have the properties ~~and relations~~ only of things which can be touched. We impose on the creations of thought the limitations of the visible and tangible.

Now, there are also other forms of perception of other senses, and these forms are perfectly analogous to space—for example, the tone-series for hearing, which

corresponds to a space of one dimension—and we do not allow ourselves a like liberty with them. We do not think of all things as sounding and do not figure to ourselves molecular events musically, in relations of heights of tones, although we are as justified in doing this as in thinking of them spatially.

This, therefore, teaches us what an unnecessary restriction we here impose upon ourselves. There is no more necessity to think of what is merely a product of thought spatially, that is to say, with the relations of the visible and tangible, than there is to think of these things in a definite position in the scale of tones.

And I will immediately show the sort of drawback that this limitation has. A system of n points is in form and magnitude determined in a space of r dimensions, if e distances between pairs of points are given, where e is given by the following table:

r	e_1	e_2
1	$n-1$	$2n-3$
2	$2n-3$	$3n-6$
3	$3n-6$	$4n-10$
4	$4n-10$	$5n-15$
5	$5n-15$	$6n-21$
r	$rn-\frac{r(r+1)}{2}$	$(r+1)n-\frac{(r+1)(r+2)}{2}$

In this table, the column marked by e_1 is to be used for e if we have made conditions about the sense of the given distances, for example, that in the straight line all points are reckoned according to one direction; in the plane all towards one side of the straight line through the first two points; in space all towards one side of the plane

through the first three points; and so on. The column marked by e_2 is to be used if merely the absolute magnitude of the distance is given.

Between n points, combining them in pairs, $\frac{n(n-1)}{1.2}$ distances are thinkable, and therefore in general more than a space of a given number of dimensions can satisfy. If, for example, we suppose the e_1-column to be the one to be used, we find in a space of r dimensions the difference between the number of thinkable distances and those possible in this space to be

$$\frac{n(n-1)}{1.2}-rn+\frac{r(r+1)}{2}=k,$$

or

$$n(n-1)-2rn+r(r+1)=2k,$$

which can be brought to the form

$$(r-n)^2+(r-n)=2k.$$

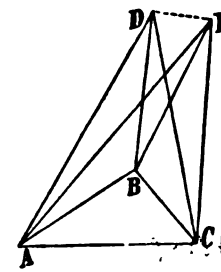
This difference is, now, zero, if

$$(r-n)^2+(r-n)=0, \text{ or } (r-n)+1=0, \text{ or } n=r+1.$$

For a space of three dimensions, the number of distances thinkable is greater than the number of distances possible in this space when the number of points is greater than four. Let us imagine, for example, a molecule consisting of five atoms, A, B, C, D, and E, then between them, ten distances are thinkable, but, in a space of three dimensions, only nine are possible, that is to say, if we choose nine such distances, the tenth thinkable one is determined by means of the nature of this space, and it is no longer arbitrary. If AB, BC, CA, AD, DB, DC, are given me, I get a tetrahedron of fixed form. If, now,

I add E with the distances $E\,A$, $E\,B$, and $E\,C$ determined, then $D\,E$ is determined by them. Thus it would be impossible gradually to alter the distance $D\,E$ without the other distances being thereby altered. Thus, there might be serious difficulties in the way of imagining many pent-atomic isomeric molecules which merely differ from one another by the relation of D and E. This difficulty vanishes in our example, when we think the pent-atomic molecule in a space of four dimensions; then ten independent distances are thinkable and also ten distances can be set up.

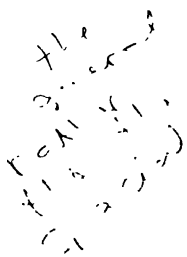
Fig. 5.



Now, the greater the number of atoms in a molecule, the higher the number of the dimensions of space do we need to make actual all the thinkable possibilities of such combinations. This is only an example to show under what limitations we proceed when we imagine the chemical elements lying side by side ~~in a space~~ of three dimensions, and how a crowd of the relations of ~~the elements~~ can escape us thereby if we wish to represent them in a formula which cannot comprise them. (See note 4, p. 86.)

It is clear how we can study the nature of chemical combinations without giving ourselves up to the conception mentioned, and how, indeed, people have now begun to study them. The heat of combustion generated by a combination gives us a clearer idea of the stability and manner of combination than any pictorial

representation. If, then, it were possible, in any molecule composed of n parts, to determine the $\frac{n(n-1)}{1.2}$ heats of combination of every two parts, the nature of the combination would be characterized thereby. According to this view, we would have to determine $\frac{n(n-1)}{1.2}$ heats of combination, whereas, if the molecules were thought spatially, $3n-6$ heats of combination suffice. Perhaps, too, a more rational manner of writing chemical combinations can be founded on this. We would write the components in a circle, draw a line from each to each, and write on the latter the respective heat of combination.



Perhaps the reason why, hitherto, people have not succeeded in establishing a satisfactory theory of electricity is because they wished to explain electrical phenomena by means of molecular events in a space of three dimensions.

Herewith I believe that I have shown that one can hold, treasure, and also turn to good account the results of modern natural science without being a supporter of the mechanical conception of nature, that this conception is not necessary for the knowledge of the phenomena and can be replaced just as well by another theory, and that the mechanical conceptions can even be a hindrance to the knowledge of phenomena.

Let me add a view on scientific theories in general: If all the individual facts—all the individual phenomena, knowledge of which we desire—were immediately accessible to us, a science would never have arisen.

Because the mental power, the memory, of the individual is limited, the material must be arranged. If, for example, to every time of falling, we knew the corresponding space fallen through, we could be satisfied with that. Only, what a gigantic memory would be needed to contain the table of the correspondences of s and t. Instead of this we remember the formula $s=\frac{gt^2}{2}$, that is to say, the rule of derivation by means of which we find, from a given t, the corresponding s, and this replaces the table just mentioned in a very complete, convenient, and compendious manner.

This rule of derivation, this formula, this "law," has, now, not in the least more real value than the aggregate of the individual facts. Its value for us lies merely in the convenience of its use: it has an economical value. (See note 5, p. 88.)

Besides this collection of as many facts as possible in a synoptical form, natural science has yet another problem which is also economical in nature. It has to resolve the more complicated facts into as few and as simple ones as possible. This we call explaining. These simplest facts, to which we reduce the more complicated ones, are always unintelligible in themselves, that is to say, they are not further resolvable. An example of this is the fact that one mass imparts an acceleration to another.

Now, it is only, on the one hand, an economical question, and, on the other, a question of taste, at what unintelligibilities we stop. People usually deceive themselves in thinking that they have reduced the

unintelligible to the intelligible. Understanding consists in analysis alone; and people usually reduce uncommon unintelligibilities to common ones. They always get, finally, to propositions of the form: if A is, B is, therefore to propositions which must follow from intuition, and, therefore, are not further intelligible.

What facts one will allow to rank as fundamental facts, at which one rests, depends on custom and on history. For the lowest stage of knowledge there is no more sufficient explanation than pressure and impact.

The Newtonian theory of gravitation, on its appearance, disturbed almost all investigators of nature because it was founded on an uncommon unintelligibility. People tried to reduce gravitation to pressure and impact. At the present day gravitation no longer disturbs anybody: it has become a *common* unintelligibility.

It is well known that action at a distance has caused difficulties to very eminent thinkers. "A body can only act where it is"; therefore there is only pressure and impact, and no action at a distance. But where is a body? Is it only where we touch it? Let us invert the matter: a body is where it acts. A little space is taken for touching, a greater for hearing, and a still greater for seeing. How did it come about that the sense of touch alone dictates to us where a body is? Moreover, contact-action can be regarded as a special case of action at a distance.

It is the result of a misconception, to believe, as people do at the present time, that mechanical facts are more intelligible than others, and that they can

provide the foundation for other physical facts. This belief arises from the fact that the history of mechanics is older and richer than that of physics, so that we have been on terms of intimacy with mechanical facts for a longer time. Who can say that, at some future time, electrical and thermal phenomena will not appear to us like that, when we have come to know and to be familiar with their simplest rules?

In the investigation of nature, we always and alone have to do with the finding of the best and simplest rules for the derivation of phenomena from one another. One fundamental fact is not at all more intelligible than another: the choice of fundamental facts is a matter of convenience, history, and custom.

The ultimate unintelligibilities on which science is founded must be facts, or, if they are hypotheses, must be capable of becoming facts. If the hypotheses are so chosen that their subject (*Gegenstand*) can never appeal to the senses and therefore also can never be tested, as is the case with the mechanical molecular theory, the investigator has done more than science, whose aim is facts, requires of him—and this work of supererogation is an evil.

Perhaps one might think that rules for phenomena, which cannot be perceived in the phenomena themselves, can be discovered by means of the molecular theory. Only that is not so. In a complete theory, to all details of the phenomenon details of the hypothesis must correspond, and all rules for these hypothetical things must also be directly transferable to the phenomenon. But then molecules are merely a valueless image.

Accordingly, we must say with J. R. Mayer: "If a fact is known on all its sides, it is, by that knowledge, explained, and the problem of science is ended."[18]

[18] *Mechanik der Wärme*, Stuttgart, 1867, p. 239.

IV

THE LOGICAL ROOT OF THE THEOREM OF EXCLUDED PERPETUAL MOTION

IF the principle of excluded perpetual motion is not based upon the mechanical view—a proposition which must be granted, since the principle was recognized before the development of this view—if the mechanical view is so fluctuating and precarious that it can give no sure foundation for this theorem, and, indeed, if it is likely that our principle is not founded on positive insight, because on it is founded the most important positive knowledge; on what does the principle rest, and whence comes its power of conviction, with which it has always ruled the greatest investigators?

I will now try to answer this question. For this purpose I must go back somewhat, to the foundations of the logic of natural science.

If we attentively observe natural phenomena, we notice that, with the variation of some of them, also variations of others occur, and in this way we have grown used to considering natural phenomena as dependent upon one another. This dependence of phenomena is called the law of causality. Now, people are accustomed to give different forms to the law of causality. Thus, for example, it is sometimes expressed: "Every effect has a cause"; which means that a variation can only occur with, or, as people prefer to say, in conse-

quence of, another. But this expression is too indefinite to be further discussed here. Besides, it can lead to great inaccuracies.

Very clearly, Fechner[19] formulated the law of causality: "Everywhere and at all times, if the same circumstances occur again, the same consequence occurs again; if the same circumstances do not occur again, the same consequence does not." By this means, as Fechner remarked farther on, "a relation is set up between the things which happen in all parts of space and at all times."

I think I must add, and have already added in another publication, that the express drawing of space and time into consideration in the law of causality, is at least superfluous. Since we only recognize what we call time and space by certain phenomena, spatial and temporal determinations are only determinations by means of other phenomena. If, for example, we express the positions of earthly bodies as functions of the time, that is to say, as functions of the earth's angle of rotation, we have simply determined the dependence of the positions of the earthly bodies on *one another.*

The earth's angle of rotation is very ready to our hand, and thus we easily substitute it for other phenomena which are connected with it but less accessible to us; it is a kind of money which we spend to avoid the inconvenient trading with phenomena, so that the proverb "Time is money" has also here a meaning. We can eliminate time from every law of nature by putting

[19] *Berichte der sächs. Ges. zu Leipzig*, Vol. II, 1850.

in its place a phenomenon dependent on the earth's angle of rotation.

The same holds of space. We know positions in space by the affection of our retina, of our optical or other measuring apparatus. And our x, y, z in the equations of physics are, indeed, nothing else than convenient names for these affections. Spatial determinations are, therefore, again determinations of phenomena by means of other phenomena.

The present tendency of physics is to represent every phenomenon as a function of other phenomena and of certain spatial and temporal positions. If, now, we imagine the spatial and temporal positions replaced in the above manner, in the equations in question, we obtain simply *every phenomenon as function of other phenomena.* (See note 6, p. 88.)

Thus the law of causality is sufficiently characterized by saying that it is the presupposition of the mutual dependence of phenomena. Certain idle questions, for example, whether the cause precedes or is simultaneous with the effect, then vanish by themselves.

The law of causality is identical with the supposition that between the natural phenomena $\alpha, \beta, \gamma, \delta, \ldots, \omega$ certain equations subsist. The law of causality says nothing about the number or form of these equations; it is the problem of positive natural investigation to determine this; but it is clear that if the number of the equations were greater than or equal to the number of the $\alpha, \beta, \gamma, \delta, \ldots, \omega$, all the $\alpha, \beta, \gamma, \delta, \ldots, \omega$ would be thereby overdetermined or at least completely determined. The fact of the varying of nature there-

fore proves that the number of the equations is less than that of the $\alpha, \beta, \gamma, \delta, \ldots, \omega$.

But with this a certain indefiniteness in nature remains behind, and I will at once call attention to it here, because I believe that even investigators of nature have sometimes overlooked it, and have thereby been led to very strange theorems. For instance, such a theorem is that defended by W. Thomson[20] and Clausius,[21] according to which after an infinitely long time the universe, by the fundamental theorems of thermodynamics, must die the death of heat, that is to say, according to which all mechanical motion vanishes and finally passes over into heat. Now such a theorem enunciated about the whole universe seems to me to be illusory throughout.

As soon as a certain number of phenomena is given, the others are co-determined, but the law of causality does not say at what the universe, the totality of phenomena, is aiming, if we may so express it, and this cannot be determined by any investigation; it is no scientific question. This lies in the nature of things.

The universe is like a machine in which the motion of certain parts is determined by that of others, only nothing is determined about the motion of the whole machine.

If we say of a thing in the universe that, after the lapse of a certain time, it undergoes the variation A, we posit it as dependent on another part of the universe,

[20] *Phil. Mag.*, October, 1852; *Math. and Phys. Papers*, I, p. 511.

[21] *Pogg. Ann.*, Bd. 93, Dezember, 1854; *Der zweite Hauptsatz d mech. Wärmetheorie*, Braunschweig, 1867.

which we consider as a clock. But if we assert such a theorem for the universe itself, we have deceived ourselves in that we have nothing over to which we could refer the universe as to a clock. For the universe there is no time. Scientific statements like the one mentioned seem to me worse than the worst philosophical ones.

People usually think that if the state of the whole universe is given at one moment, it is completely determined at the next one; but an illusion has crept in there. This next moment is given by the advance of the earth. The position of the earth belongs to the circumstances. But we easily commit the error of counting the same circumstance twice. If the earth advances, this and that occur. Only the question as to *when* it will have advanced has no meaning at all. The answer can be given only in the form: It has advanced farther then, if it has advanced farther.

It may not be unimportant for the investigator of nature to consider and recognize the ~~indetermination which the law of causality leaves over.~~ To be sure, the only value of this for him is to keep him from transgressing its limits. On the other hand, an idle philosopher could perhaps connect his ideas on freedom of the will with this, with better luck than he has had hitherto in the case of other gaps in knowledge. (See note 7, p. 90.) For the investigator of nature there is nothing else to find out but the dependence of phenomena on one another.

Let us call the totality of the phenomena on which a phenomenon a can be considered as dependent, *the cause*. If this totality is given, a is determined, and

determined uniquely. Thus the law of causality may also be expressed in the form: "The effect is determined by the cause."

This last form of the law of causality may well have been that which was already in existence at a very low stage of human culture, and yet existed in full clearness. In general, a lower stage of knowledge may perhaps be distinguished from a higher one not so much by the difference of the conception of causality as by the manner of application of this conception.

He who has no experience will, because of the complication of the phenomena surrounding him, easily suppose a connexion between things which have no perceptible influence on one another. Thus, for example, an alchemist or wizard may easily think that, if he cooks quicksilver with a Jew's beard and a Turk's nose at midnight at a place where roads cross, while nobody coughs within the radius of a mile, he will get gold from it. The man of science of to-day knows from experience that such circumstances do not alter the chemical nature of things, and accordingly he has a smoother path to traverse. Science has grown almost more by what it has learned to ignore than by what it has had to take into account.

If we call to remembrance our early youth, we find that the conception of causality was there very clearly, but not the correct and fortunate application of it. In my own case, for example—I remember this exactly—there was a turning-point in my fifth year. Up to that time I represented to myself everything which I did not understand—a pianoforte, for instance—as simply a

motley assemblage of the most wonderful things, to which I ascribed the sound of the notes. That the pressed key struck the cord with the hammer did not occur to me. Then one day I saw a wind-mill. I saw how the cogs on the axle engaged with the cogs which drive the mill-stones, how one tooth pushed on the other; and, from that time on, it became quite clear to me that all is not connected with all, but that, under circumstances, there is a choice. At the present time, every child has abundant opportunities for making this step. But there was a time, as the epidemic of belief in witches, which belief lasted many centuries, proves, in which this step was only permitted to the greatest minds.

By this I only wanted to show that, without positive experiences, the law of causality is empty and barren. This appears still better with another theorem, which we recognize at once as an inverse of the law of causality —with the law of sufficient reason. Let us explain this law by some examples.

Let us take a straight horizontal bar, which we support in its middle and at both ends of which we hang equal weights. Then we perceive at once that equilibrium must subsist, because there is no reason why the bar should turn in one direction rather than in the other. So Archimedes concluded.

If we let four equal forces act at the centre of gravity of a regular tetrahedron in the directions of its vertices, equilibrium reigns. Again there is no reason why motion should result in one direction rather than in another.

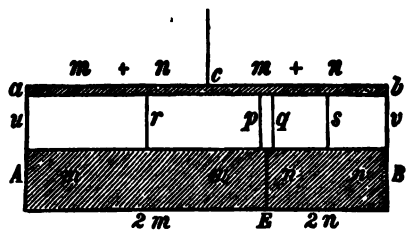
Only this is not expressed quite properly: we ought rather to say that there is a reason that, in these cases, *nothing* happens. For the effect is determined by the cause, and the one and only effect which is here determined by the cause is *no* effect at all. In fact, if any effect were to occur, no rule of derivation of it from the circumstances could be given. If, for example, we imagine any resultant in the above tetrahedron of forces and set up a rule for its derivation, there are eleven other resultants which can be found by the same rule. Consequently, nothing is determined. The one and only effect which is determined in this case is the effect which is equal to zero. The law of sufficient reason is not essentially different from the law of causality or from the theorem: "The effect is determined by the cause."

But how is a person who has made no experiments to apply this theorem? Give him a lever with arms of equal length and with its ends loaded with equal weights, but with the weights and arms of different colours and forms. Without experimental knowledge, he will never discover those circumstances which alone are relevant. As an example of how important experience is in such derivations, I will give Galileo's demonstration of the law of the lever. Galileo borrowed it from Stevinus and slightly modified it, and Stevinus somewhat varied Archimedes's demonstration.

A horizontal prism $A\,B$ is hung at the ends by two threads u and v on to a horizontal bar $a\,b$, which can be rotated about its middle c, or is hung up there by a thread. Such a system is, as we see at once, in equilib-

rium. If, now, we divide the prism into two parts of lengths $2m$ and $2n$ by a section at E, after we have attached two new threads p and q at both sides of the section, equilibrium still subsists. It will also still subsist if we hang the piece $A\,E$ in its middle by the thread r, and $E\,B$ by s to $a\,b$, and take away p, q, u, v. But then at a distance n from c hangs a prism of weight

Fig. 6.



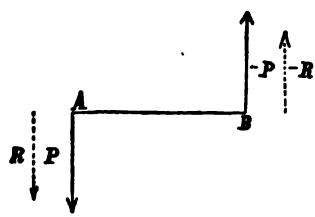
$2m$, and at a distance m from c hangs a prism of weight $2n$. Now the practical physicist knows that the tension of the threads, which alone mediates between the prism and the bar, depends only on the magnitude, and not on the form, of the weight. Therefore we can, again without disturbance of the equilibrium, replace the pieces of the prism by any other weights $2m$ and $2n$; and this gives the known law of the lever.

Now, he who had not had a great deal of experience in mechanical things certainly could not have carried out such a demonstration.

Yet another example. At A and B are the equal and parallel forces P and $-P$ to act. As is well known, they have no resultant. Let us suppose, for example, that $-R$ is a resultant, then we must also suppose that

R is one, for it is determined by the same rule as $-R$, if we turn round the figure through two right angles. Consequently, the one and only resultant completely determined by the circumstances is, in this case, *no* resultant. However, this holds only if we know already that we have to seek the resultant in the plane of symmetry of the system, that is to say, in the plane of P and $-P$, and that the forces P and $-P$ have no lateral effect. But apart from this, a resultant is at once unambiguously determined by the following rule, for example. Place yourself so that your feet are at one of the points A, B, with your head in the direction of the force acting there, and look towards the other point, drawing the resultant towards the right, perpendicularly to the plane $(P, -P)$. In fact, the part of line thus determined has a signification for our case. It is, however, not the resultant, but the axis of the Poinsot's couple represented in Fig. 7.

Fig. 7.



If P and $-P$ were not simple forces, but the axes of a Poinsot's couple—if we had, consequently, things affected with a certain lateralness—the direction just determined would represent the direction of the resultant motion, if we choose the axes so that, for an observer with his head at the arrow-head and his feet at B, the rotation takes place in the plane through B perpendicular to $-P$ in the direction of the hands of a clock.

Now, whether the things we have to consider have

such a lateralness often cannot be determined at the first glance, but can only be so determined by means of much experience. The lateralness of light remained hidden for a long time, and caused great surprise to its discoverer Malus. If an electric current flows in the vertical plane drawn through a magnetic needle, from the south pole towards the north pole, one thinks that all is symmetrical with respect to this plane and that the needle could, at most, move in this plane. One is greatly surprised when one hears for the first time that the north pole deviates to the left of a swimmer in the current, who is looking at the needle.

The law of sufficient reason is an excellent instrument in the hands of an experienced investigator, but is an empty formula in the hands of even the most talented people in whom special knowledge is lacking.

After these considerations, now, it will not be hard for us to discover the source from which the principle of excluded perpetual motion arises. It is again only another form of the law of causality.

"It is not possible to create work out of nothing." If a group of phenomena is to become the source of continual work, this means that it shall become a source of continual variation of another group of phenomena. For, by means of the general connexion of nature, all phenomena are also connected with mechanical phenomena, and therefore with the performance of work. Every source of continual variation of phenomena is a source of work, and inversely.

If, now, the phenomena α, β, γ, . . . depend on the phenomena x, y, z, . . . , certain equations

$$\alpha = f_1(x, y, z, \ldots),$$
$$\beta = f_2(x, y, z, \ldots),$$
$$\gamma = f_3(x, y, z, \ldots),$$

subsist, from which α, β, γ, . . . are uniquely determined when x, y, z, . . . are given. Now, it is clear that:

1. As long as x, y, z, . . . are constant, α, β, γ, . . . are;

2. If x, y, z, . . . make merely one step, so do α, β, γ, . . . ;

3. If x, y, z, . . . vary periodically, so do α, β, γ, . . . ;

4. If, finally, α, β, γ, . . . are to undergo continual variations, x, y, z, . . . must necessarily do so.

If a group of phenomena x, y, z, . . . is to become a source of work, a source of the continual variation of another group α, β, γ, . . . , the group x, y, z, . . . itself must be engaged in continual variation. This is a clear form of the theorem of excluded perpetual motion, and one which cannot be misinterpreted. In this abstract form the theorem has nothing to do with mechanics particularly, but can be applied to all phenomena. The theorem of excluded perpetual motion is merely a special case of the theorem here enunciated.

The remark which has been made cannot be inverted. In general, certain systems of continual variations of x, y, z, . . . , which make no difference to α, β, γ, . . . can be imagined, that is to say, groups of appearances can be given, which are engaged in continual variation without being sources of continual variation

of other groups of phenomena. These are groups shut up in themselves. How such groups can be divided, that is to say, which phenomena depend on one another and in what manner, and which do not, can be taught only by experience, and the law of causality says nothing about it.

The theorem of excluded perpetual motion, without positive experience, is just as empty as the law of sufficient reason and all formal laws of that kind. On this account—and history teaches this—it has found more and more applications in physics as positive knowledge progressed. First it was applied in mechanics alone, then in the theory of heat, and lastly in the theory of electricity. Abstract theorems alone lead to nothing; and Poinsot[22] remarked very correctly: "Rien ne vous dispense d'étudier les choses en elles-mêmes, et de nous bien rendre compte des idées qui font l'objet de nos speculations."

Let us illustrate the theorem of excluded perpetual motion by some examples.

The vibrations of a tuning-fork are periodical variations; they can become a lasting source of work only if they themselves undergo lasting variations—for example, by the diminution of their amplitude. We hear a tuning-fork only because its vibrations thus decrease.

A rotating top can perform work if its angular velocity decreases.

The mere lying side by side of a copper and a zinc plate will generate no electric *current*. From where,

[22] *Théorie nouvelle de la rotation des corps*, Paris, 1851, p. 80.

indeed, would the continual variation come if the plates themselves underwent no such variation? But if a continual chemical change of the plates occurs, we have no further objection to make against the supposition of an electric current.

An example of the unpermissibility of the process of inversion mentioned above is as follows: A top which is protected from resistance can rotate uniformly without becoming a source of work. Its angular velocity remains constant, but its angle of rotation varies continually. This does not contradict the principle. But experience adds—what the principle does not know—that in this case only variations of velocity, and not variations of position, can become a source of other variations But if one were to think that the top's continual variation of position is connected with no other continual variation, it would again be a mistake. It is connected with the increasing angle of rotation of the earth. This view leads, to be sure, to a peculiar conception of the law of inertia, into the further discussion of which we shall not enter.

Though the principle of excluded perpetual motion is very fruitful in the hands of an experienced investigator, it is useless in a department of experience which has not been accurately explored.

People have put a special value on the fact that the sum of the store of the work at our disposal and the *vis viva*, or the energy, is constant. Only, although we must admit that such a commercial or housekeeping expression is very convenient, easily seized, and suitable to human nature, which is planned throughout on

economical grounds, we find, on looking into the matter quietly and accurately, that there is nothing essentially more in such a law than in any other law of nature.

The law of causality supposes a dependence between the natural phenomena $\alpha, \beta, \gamma, \ldots$. It is the problem of the investigator of nature to find out the manner of this dependence. Now, it does not matter very much how the equations representing this dependence are written. All will agree that it makes no great difference in which of the three forms an equation is written,

$$f(\alpha, \beta, \gamma, \ldots)=0,\ \alpha=\psi(\beta, \gamma, \ldots),\ F(\alpha, \beta, \gamma, \ldots)=\text{const.},$$

and that in the last of these forms there lies no specially higher wisdom than in the others.

But it is merely by this form that the law of the conservation of work differs from other laws of nature. We can easily give a similar form to any other law of nature; thus, we can write Mariotte's law, where p is the force of expansion and v is the volume of the unit of mass, in the form $\log p+\log v=\text{const.}$ However beautiful, simple, and perspicuous much in the form of the theorem of the conservation of work looks, I cannot feel any enthusiasm for the mysticism which some people love to push forwards by means of this theorem.

By this I believe that I have shown that the theorem of excluded perpetual motion is merely a special form of the law of causality, which law results immediately from the supposition of the dependence of phenomena on one another—a supposition which precedes every scientific investigation; and which is quite unconnected with the mechanical view of nature, but

is consistent with any view, if only it firmly retains a strict rule by laws.

We have, on this occasion, seen that the riches which investigators of nature have, in the course of time, heaped up by their work are of very different kinds. They are in part actual pieces of knowledge, in part also superseded theories, great and small, points of view that were now and then useful at an earlier stage, but are now irrelevant, philosophemes—among them some of the worst kind, by which some people wrongly condemn investigators of nature—and so on. It can only be useful sometimes to hold a review of these treasures; and this gives us the opportunity of putting aside what is worthless, and one does not run the risk of confusing deeds of assignment with property.

The object of natural science is the connexion of phenomena; but the theories are like dry leaves which fall away when they have long ceased to be the lungs of the tree of science.

NOTES

1. (See p. 28.) The law of inertia was afterwards formulated by Newton in the following way:

"Corpus omne perseverare in statu suo quiescendi vel movendi uniformiter in directum nisi quatenus a viribus impressis cogitur statum illum mutare."

Philosophiae Naturalis Principia Mathematica, Amstaelodami, 1714, Tom. I, p. 12 (Lex. I of the "Axiomata sive leges motus"); cf. pp. 2, 358. [The first edition of the *Principia* was published in London in 1687, the second edition at Cambridge in 1713, the third in London in 1726, and an English translation, in two volumes, by Andrew Motte, in London, 1729 (American editions, New York, 1848 and 1850, one vol.). Full bibliographical information as to the various editions and translations of Newton's works is given in George J. Gray's book, *A Bibliography of the Works of Sir Isaac Newton*, 2d ed., Cambridge, 1907.]

Since Newton, this law, which was with Galileo a mere remark, has attained the dignity and intangibleness of a papal dictum. Perhaps the best way to enunciate it is: Every body keeps its direction and velocity as long as they are not altered by outer forces.

Now, I remarked many years ago that there is in this law a great indefiniteness; for which body it is, with respect to which the direction and velocity of the body in motion is determined, is not stated. I first drew attention to this indefiniteness, to a series of paradoxes which can be deduced from it, and to the solution of the difficulty, in my course of lectures "Ueber einige Haupt-

fragen der Physik" in the summer of 1868, before an audience of about forty persons. I referred regularly to the same subject in the years following, but my investigation was not printed for reasons stated in the next note.

Now, a short while ago, C. Neumann[1] discussed this point, and found exactly the same indefiniteness, difficulties, and paradoxes in the law. Although I was sorry to have lost the priority in this important matter, yet the exact coincidence of my views with those of so distinguished a mathematician gave me great pleasure and richly compensated me for the disdain and surprise which almost all the physicists with whom I discussed this subject showed. Also, I think that I may, without fear, assert my independence in a matter of which I spoke before so large an audience and so long before.

Now, I must add that, although the difficulties which I found in the law of inertia exactly coincide with those of Neumann, yet my solution of them is different. Neumann thought that he had removed the difficulties by considering all motion as absolute and determined by means of a hypothetical body a. Only then everything remains as it was of old. The law of inertia apparently receives a more distinct enunciation, but it did not turn out differently in practice. This appears from the following considerations.

Obviously it does not matter whether we think of the earth as turning round on its axis, or at rest while the celestial bodies revolve round it. Geometrically these

[1] [*Ueber die Principien der Galilei-Newton'schen Theorie* Leipzig, 1870.]

are exactly the same case of a relative rotation of the earth and of the celestial bodies with respect to one another. Only, the first representation is astronomically more convenient and simpler.

But if we think of the earth at rest and the other celestial bodies revolving round it, there is no flattening of the earth, no Foucault's experiment, and so on—at least according to our usual conception of the law of inertia. Now, one can solve the difficulty in two ways: Either all motion is absolute, or our law of inertia is wrongly expressed. Neumann preferred the first supposition, I, the second. The law of inertia must be so conceived that exactly the same thing results from the second supposition as from the first. By this it will be evident that, in its expression, regard must be paid to the masses of the universe.

In ordinary terrestrial cases, it will answer our purposes quite well to reckon the direction and velocity with respect to the top of a tower or a corner of a room; in ordinary astronomical cases, one or other of the stars will suffice. But because we can also choose other corners of rooms, another pinnacle, or other stars, the view may easily arise that we do not need such a point at all from which to reckon. But this is a mistake; such a system of co-ordinates has a value only if it can be determined by means of bodies. We here fall into the same error as we did with the representation of time. Because a piece of paper money need not necessarily be funded by a definite piece of money, we must not think that it need not be funded at all.

In fact, any one of the above points of origin of co-

ordinates answers our purposes as long as a sufficient number of bodies keep fixed positions with respect to one another. But if we wish to apply the law of inertia in an earthquake, the terrestrial points of reference would leave us in the lurch, and, convinced of their uselessness, we would grope after celestial ones. But, with these better ones, the same thing would happen as soon as the stars showed movements which were very noticeable. When the variations of the positions of the fixed stars with respect to one another cannot be disregarded, the laying down of a system of co-ordinates has reached an end. It ceases to be immaterial whether we take this or that star as point of reference; and we can no longer reduce these systems to one another. We ask for the first time which star we are to choose, and in this case easily see that the stars cannot be treated indifferently, but that because we can give preference to none, the influence of all must be taken into consideration.

We can, in the application of the law of inertia, disregard any particular body, provided that we have enough other bodies which are fixed with respect to one another. If a tower falls, this does not matter to us; we have others. If Sirius alone, like a shooting-star, shot through the heavens, it would not disturb us very much; other stars would be there. But what would become of the law of inertia if the whole of the heavens began to move and the stars swarmed in confusion? How would we apply it then? How would it have to be expressed then? We do not inquire after one body as long as we have others enough; nor after

one piece of money as long as we have others enough. Only in the case of a shattering of the universe, or a bankruptcy, as the case may be, we learn that *all* bodies, each with its share, are of importance in the law of inertia, and all money, when paper money is funded, is of importance, each piece having its share.

Yet another example: A free body, when acted upon by an instantaneous couple, moves so that its central ellipsoid with fixed centre rolls without slipping on a tangent-plane parallel to the plane of the couple. This is a motion in consequence of inertia. Here the body makes very strange motions with respect to the celestial bodies. Now, do we think that these bodies, without which one cannot describe the motion imagined, are without influence on this motion? Does not that to which one must appeal explicitly or implicitly when one wishes to describe a phenomenon belong to the most essential conditions, to the causal nexus of the phenomenon? The distant heavenly bodies have, in our example, no influence on the acceleration, but they have on the velocity.

Now, what share has every mass in the determination of direction and velocity in the law of inertia? No definite answer can be given to this by our experiences. We only know that the share of the nearest masses vanishes in comparison with that of the farthest. We would, then, be able completely to make out the facts known to us if, for example, we were to make the simple supposition that all bodies act in the way of determination proportionately to their masses and independently of the distance, or proportionately to the

distance, and so on. Another expression would be: In so far as bodies are so distant from one another that they contribute no noticeable acceleration to one another, all distances vary proportionately to one another.

I will return to the subject on another occasion.

2. (See p. 29.) Perhaps I may mention here that I tried to get my bearings with respect to the concept of mass by the help of the principle of excluded perpetual motion. My note on this subject was returned as unusable by Poggendorff, the then editor of the *Annalen der Physik und der Chemie*, after he had had it about a year, and it appeared later in the fourth volume of Carl's *Repertorium*.[2] This rejection was also the reason why I did not publish my investigations on the law of inertia. If I ran up against the physics of the schools in so simple and clear a matter, what could I expect in a more difficult question? The *Annalen* often contain pages of fallacies about Torricelli's theorem and the blush of dawn—written, to be sure, in "physical language"; but the inclusion of a short note which is not wholly written in that jargon would obviously greatly lower the value of the *Annalen* in the eyes of the public.

The following is a complete reproduction of the note in question:

ON THE DEFINITION OF MASS

The circumstance that the fundamental propositions of mechanics are neither wholly *a priori* nor can wholly be discovered by means of experience—for sufficiently numerous and

[2] *Ueber die Definition der Masse, Repertorium für physikalische Technik* , Bd. IV, 1868, pp. 355 sqq.

accurate experiments cannot be made—results in a peculiarly inaccurate and unscientific treatment of these fundamental propositions and conceptions. Rarely is distinguished and stated clearly enough what is *a priori*, what empirical, and what is hypothesis.

Now, I can only imagine a scientific exposition of the fundamental propositions of mechanics to be such that one regards these theorems as hypotheses to which experience forces us, and that one afterwards shows how the denial of these hypotheses would lead to contradictions with the best-established facts.

As evident *a priori* we can only, in scientific investigations, consider the law of causality or the law of sufficient reason, which is only another form of the law of causality. No investigator of nature doubts that under the same circumstances the same always results, or that the effect is completely determined by the cause. It may remain undecided whether the law of causality rests on a powerful induction or has its foundation in the psychical organization (because in the psychical life, too, equal circumstances have equal consequences).

The importance of the law of sufficient reason in the hands of an investigator was proved by Clausius's works on thermodynamics and Kirchhoff's researches on the connexion of absorption and emission. The well-trained investigator accustoms himself in his thought, by the aid of this theorem, to the same definiteness as nature has in its actions, and then experiences which are not in themselves very apparent suffice, by exclusion of all that is contradictory, to discover very important laws connected with the said experiences.

Usually, now, people are not very chary of asserting that a proposition is immediately evident. For example, the law of inertia is often stated to be such a proposition, as if it did not need the proof of experience. The fact is that it can only have grown out of experience. If masses imparted to one another, not acceleration, but, say, velocities which depended on the distance, there would be no law of inertia; but whether we have the one state of things or the other, only experience teaches. If we had merely sensations of heat, there would be merely

equalizing velocities (*Ausgleichungsgeschwindigkeiten*), which vanish with the differences of temperature.

One can say of the motion of masses: "The effect of every cause persists," just as correctly as the opposite: "Cessante causa cessat effectus"; it is merely a matter of words. If we call the resulting velocity the "effect," the first proposition is true, if we call the acceleration the "effect," the second is true.

Also people try to deduce *a priori* the theorem of the parallelogram of forces; but they must always bring in tacitly the supposition that the forces are independent of one another. But by this the whole derivation becomes superfluous.

I will now illustrate what I have said by *one* example, and show how I think the conception of mass can be quite scientifically developed. The difficulty of this conception, which is pretty generally felt, lies, it seems to me, in two circumstances: (1) in the unsuitable arrangement of the first conceptions and theorems of mechanics; (2) in the silent passing over important presuppositions lying at the basis of the deduction.

Usually people define $m=\frac{p}{g}$ and again $p=mg$. This is either a very repugnant circle, or it is necessary for one to conceive force as "pressure." The latter cannot be avoided if, as is customary, statics precedes dynamics. The difficulty, in this case, of defining magnitude and direction of a force is well known.

In that principle of Newton, which is usually placed at the head of mechanics, and which runs: "Actioni contrariam semper et aequalem esse reactionem: sive corporum duorum actiones in se mutuo semper esse aequales et in partes contrarias dirigi," the "actio" is again a pressure, or the principle is quite unintelligible unless we possess already the conception of force and mass But pressure looks very strange at the head of the quite phoronomical mechanics of today. However, this can be avoided.

If there were only one kind of matter, the law of sufficient reason would be sufficient to enable us to perceive that two completely similar bodies can impart to each other only *equal* and

opposite accelerations. This is the one and only effect which is completely determined by the cause.

Now, if we suppose the mutual independence of forces, the following easily results. A body A, consisting of m bodies a, is in the presence of another body B, consisting of m' bodies a. Let the acceleration of A be ϕ and that of B be ϕ'. Then we have $\phi:\phi'=m':m$.

If we say that a body A has the mass m if it contains the body a m times, this means that the accelerations vary as the masses.

To find by experiment the mass-ratio of two bodies, let us allow them to act on one another, and we get, when we pay attention to the sign of the acceleration, $\frac{m}{m'}=-\left(\frac{\phi'}{\phi}\right)$.

If the one body is taken as unit of mass, the calculation gives the mass of the other body. Now, nothing prevents us from applying this definition in cases in which two bodies of different matter act on one another. Only, we cannot know *a priori* whether we do not obtain other values for a mass when we consult other bodies used for purposes of comparison and other forces. When it was found that A and B combine chemically in the ratio $a:b$ of their weights and that A and C do so in the ratio $a:c$ of their weights, it could not be known beforehand that B and C combine in the ratio $b:c$. Only experience can teach us that two bodies which behave to a third as equal masses will also behave to one another as equal masses.

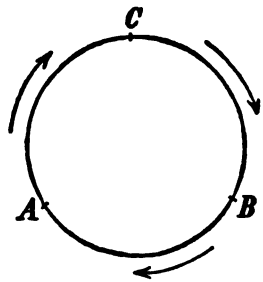
If a piece of gold is opposed to a piece of lead, the law of sufficient reason leaves us completely. We are not even justified in expecting contrary motions: both bodies might accelerate in the same direction. The calculation would then lead to negative masses.

But that two bodies, which behave as equal masses to a third, behave as such to one another with respect to any forces, is very likely, because the contrary would not be reconcilable with the law of the conservation of work (*Kraft*), which has hitherto been found to be valid.

Imagine three bodies A, B, and C movable on an absolutely

smooth and absolutely fixed ring. The bodies are to act on one another with any forces. Further, both A and B, on the one hand, and A and C, on the other, are to behave to one another as equal masses. Then the same must hold between B and C. If, for example, C behaved to B as a greater mass to a lesser one, and we gave B a velocity in the direction of the arrow, it would give this velocity wholly to A by impact, and A would give it wholly to C. Then C would communicate to B a greater velocity and yet keep some itself. With every revolution in the direction of the arrow, then, the *vis viva* in the ring would increase; and the contrary would take place if the original motion were in a direction opposite to that of the arrow. But this would be in glaring contradiction with the facts hitherto known.

Fig. 8.



If we have thus defined mass, nothing prevents us from keeping the old definition of force as product of mass and acceleration. The law of Newton mentioned above then becomes a mere identity.

Since all bodies receive from the earth an equal acceleration, we have in this force (their weight) a convenient measure of their masses, again, however, only under the two suppositions that bodies which behave as equal masses to the earth do so to one another, and with respect to every force. Consequently, the following arrangement of the theorems of mechanics would appear to me to be the most scientific.

Theorem of experience.—Bodies placed opposite to one another communicate to each other accelerations in opposite senses in the direction of their line of junction. The law of inertia is included in this.

Definition.—Bodies which communicate to each other equal and opposite accelerations are said to be of equal mass. We get the mass-value of a body by dividing the acceleration which

it gives the body with which we compare others, and choose as the unit, by the acceleration which it gets itself

Theorem of experience.—The mass-values remain unaltered when they are determined with reference to other forces and to another body of comparison which behaves to the first one as an equal mass.

Theorem of experience.—The accelerations which many masses communicate to one another are mutually independent. The theorem of the parallelogram of forces is included in this.

Definition.—Force is the product of the mass-value of a body into the acceleration communicated to that body.

PRAGUE
November 15, 1867

3. (See p. 47.) The note in question appeared in the number for February, 1871, of the Prague journal, *Lotos*, but was, however, drawn up a year earlier. This is a complete reproduction of it:

The second law of thermodynamics can, as is well known, be expressed for a simple case by the equation

$$-\frac{Q}{T}+Q'\left(\frac{1}{T'}-\frac{1}{T}\right)=0,$$

where Q denotes the quantity of heat transformed into work, at the absolute temperature T, and Q' the quantity of heat which simultaneously sunk from the higher temperature T to the temperature T'.

Now, we have not far to seek for the observation that this theorem is not limited to the phenomena of heat, but can be transferred to other natural phenomena, if, instead of the quantity of heat, we put the potential of whatever is active in the phenomenon, and, instead of the absolute temperature, the potential function. Then the theorem may be expressed thus:

If a certain potential-value P of an agent at the potential-level V passes over into another form—for example, if the potential of an electrical discharge is transferred into heat—then another potential-value, P', of the same agent sinks simultaneously

from the higher potential-level V to the lower one V'. And the said values are connected with one another by the equation

$$-\frac{P}{V}+P'\left(\frac{1}{V'}-\frac{1}{V}\right)=0.$$

In the application of the theorem, the only questions are, what is to be conceived as potential (as equivalent of mechanical work), and what is the potential-function. In many cases this is self-evident and long established, in others it can easily be found. If, for example, we wish to apply the theorem to the impact of inert masses, obviously the *vis viva* of these masses is to be conceived as the potential, and their velocity as the potential-function. Masses of equal velocity can communicate no *vis viva* to one another—they are at the same potential-level.

I must reserve for another occasion the development of these theorems.

PRAGUE
February 16, 1870

4. (See p. 53.) The manner in which I was led to the view that we need not necessarily represent to ourselves molecular-processes spatially, at least not in a space of three dimensions, was as follows:

In the year 1862, I drew up a compendium of physics for medical men, in which, because I strove after a certain philosophical satisfaction, I carried out rigorously the mechanical atomic theory. This work first made me conscious of the insufficiency of this theory, and this was clearly expressed in the preface and at the end of the book, where I spoke of a total reformation of our views on the foundations of physics.

I was busied, at the same time, with psychophysics and with Herbart's works, and so I became convinced that the intuition of space is bound up with the organization of the senses, and, consequently, that we are not

justified in ascribing spatial properties to things which are not perceived by the senses. In my lectures on psychophysics,[3] I already stated clearly that we are not justified in thinking of atoms spatially. Also, in my theory of the organ of hearing,[4] I brought before my readers the series of tones as an analogue of space of one dimension. At the same time the quite arbitrary and, on this account, faulty limitation of the number of dimensions in Herbart's derivation of "intelligible" space struck me. By that, now, it became clear to me that, for the understanding, relations like those of space, and of any number of dimensions, are thinkable.

My attempts to explain mechanically the spectra of the chemical elements and the divergence of the theory with experience strengthened my view that we must not represent to ourselves the chemical elements in a space of three dimensions. I did not venture, however, to speak of this candidly before orthodox physicists. My notices in Schlömilch's *Zeitschrift* of 1863 and 1864 contained only an indication of it.

All the views on space and time developed in this pamphlet were first communicated in my course of lectures on mechanics in the summer of 1864 and in my course on psychophysics delivered in the winter of 1864–1865, which latter course was attended by large audiences, and also by many professors of the University of Graz. The most important and most general results of these considerations were published by me in the form of short notes in Fichte's *Zeitschrift für Philosophie* of

3 *Oesterr. Zeitschr. für praktische Heilkunde*, 1863.

4 *Sitzber. der Wiener Akademie*, 1863.

1865 and 1866. In this, external stimuli were entirely lacking, for Riemann's paper, which first appeared in 1867,[5] was quite unknown to me.

5. (See p. 55.) The view that in science we are chiefly concerned with the convenience and saving of thought, I have maintained since the beginning of my work as a teacher. Physics, with its formulae and potential-function, is especially suited to put this clearly before me. The moment of inertia, the central ellipsoid, and so on, are simply examples of substitutes by means of which we conveniently save ourselves the consideration of the single mass-points. I also found this view developed with especial clearness in the case of my friend the political economist E. Herrmann. From him I have taken what seems to me a very suitable expression: "Science has a problem of economy or thrift."

6. (See p. 61.) From my essay on the development of presentations of space in Fichte's *Zeitschrift* for 1866,[6] I permit myself to extract the following passage:

> Now, I think that we can go still farther in the scale of presentations of space and thus attain to presentations whose totality I will call *physical space.*
>
> It cannot be my intention here to criticize our conceptions of matter, whose insufficiency is, indeed, generally felt. I will merely make my thoughts clear. Let us imagine, then, a something behind (*unter*) matter in which different states can occur;

[5] [Riemann's work *Ueber die Hypothesen, welche der Geometrie zu Grunde liegen* was written and read to a small circle in 1854, first published posthumously in 1867, and reprinted in his *Ges. Werke*, pp. 255–268.]

[6] "Ueber die Entwicklung der Raumvorstellungen," *Zeitschr. für Philosophie und philosophische Kritik*, 1866.

say, for simplicity, a pressure in it, which can become greater or smaller.

Physics has long been busied in expressing the mutual action, the mutual attraction (opposite accelerations, opposite pressures) of two material particles as a function of their distance from each other—therefore of a spatial relation. Forces are functions of the distance. But now, the spatial relations of material particles can, indeed, only be recognized by the forces which they exert on one another.

Physics, then, does not strive, in the first place, after the discovery of the fundamental relations of the various pieces of matter, but after the derivation of relations from other, already given, ones. Now, it seems to me that the fundamental law of force in nature need not contain the spatial relations of the pieces of matter, but must only state a dependence between the states of the pieces of matter.

If the positions in space of the material parts of the whole universe and their forces as functions of these positions were once known, mechanics could give their motions completely,[7] that is to say, it could make all the positions discoverable at any time, or put down all positions as functions of time.

But, what does time mean when we consider the universe? This or that "is a function of time" means that it depends on the position of the vibrating pendulum, on the position of the rotating earth, and so on. Thus, "All positions are functions of time" means, for the universe, that all positions depend upon one another.

But since the positions in space of the material parts can be recognized only by their states, we can also say that all the states of the material parts *depend upon one another.*

The physical space which I have in mind—and which, at the same time, contains time in itself—is thus nothing other than *dependence of phenomena on one another.* A complete physics, which would know this fundamental dependence, would have

[7] [For this purpose, it would be necessary also to know the *velocities* of the various parts at that instant.—TR.]

no more need of special considerations of space and time, for these latter considerations would already be included in the former knowledge.

My researches on the time-sense of the ear[8] contain the following passage:

Physics sets out to represent every phenomenon as a function of time. The motion of a pendulum serves as the measure of time. Thus, physics really expresses every phenomenon as a function of the length of the pendulum. We may remark that this also happens when forces, say, are represented as functions of the distance; for the conception of force (acceleration) already contains that of time. If one were to succeed in expressing every phenomenon—physical and psychical—as a function of the phenomenon of pendulum-motion, this would only prove that all phenomena are so connected that any one of them can be represented as a function of any other. Physically, then, time is the representability of any phenomenon as a function of any other one.

This view of time, now, also plays a part in my discussion of the law of inertia. To this view, too, Neumann, in his discussion of the law of inertia, seems to incline.

7. (See p. 63.) Fechner believed that he could reconcile the law of causality with the freedom of the will, in the following manner:

It is at once evident that our law, in spite of the fact that it would be binding for all space and all time, for all matter and all spirit, yet, in its essence, leaves behind an indetermination—indeed, the greatest that can be imagined. For it says, to be sure, that, if the same circumstances occur again, the same consequence must occur again, and if not, not; but there is nothing in its expression to determine in any way the manner

[8] "Ueber den Zeitsinn des Ohres," *Sitzb. der Wien. Akad.*, 1865

of the first consequence at any place and with any circumstances, nor the manner of the occurrence of the first circumstances themselves.

Farther on, Fechner remarked that the same circumstances never occur again, nor, therefore, ever exactly the same consequences.

As regards the first point, the indefiniteness is put back to the moment of creation, but the second seems to me to be merely an indeterministic subterfuge.

The indefiniteness to which I have drawn attention is essentially different; it is always present and results immediately from the law of causality by the elimination of space and time.

GENERAL REMARKS

We learn very soon to distinguish our presentations from our sensations (perceptions). Now, the problem of science can be split into three parts:

1. The determination of the connexion of presentations. This is psychology.

2. The discovery of the laws of the connexion of sensations (perceptions). This is physics.

3. The clear establishment of the laws of the connexion of sensations and presentations. This is psychophysics.

If we think of the laws of connexion as mathematical, the establishment of those laws presupposes the measurability of all that they embrace. In that there still remains, to be sure, much to be desired. Fechner, in his *Psychophysik*, succeeded in measuring even the single sensations, but it is possible to be in doubt

as to the meaning of this measure. A sensation of greater intensity is always also of another quality, and then Fechner's measure is more physical than psychical. However, these difficulties turn out to be not insurmountable.

AUTHOR'S NOTES TO THE SECOND EDITION (1909)

To p. 19.—The confusion caused by the use of the expression "force" in a different signification is also shown in a communication of Faraday's of 1857 (*Phil. Mag.*, Ser. 4, Vol. XIII, p. 225 [in a paper "On the Conservation of Force"; also *Proc. Roy. Inst.*, February 27, 1857]). The same fault was committed by many of the most eminent investigators of that time. [Cf. also *Wärmelehre*, p. 206; and, on the history of the use of such terms as "work" and "energy," cf. A. Voss, *Encykl. der math. Wiss.*, IV, 1, 1901, pp. 102–104; and Mach, *Mechanics*, p. 499, note.]

To pp. 28 and 75.—The question of the law of inertia was treated at length in my *Mechanics* [pp. 140–141, 142–143, 523–525, 542–547, 560–574], where all the literature of the subject is noticed. The last important work that is known to me is J. Petzoldt's article, "Die Gebiete der absoluten und relativen Bewegung" (Ostwald's *Annalen der Naturphilosophie*, VII, p. 29).

To pp. 29 and 80.—Further developments in my *Mechanics* [pp. 194–197, 198–222, 243, 536–537, 539–540, 555–560].

To pp. 35–37, 47, 85–86.—The publications which contain analogous considerations—partly coincident, partly allied—are my *Mechanics;* Josef Popper, *Die physikalischen Grundsätze der elektrischen Kraftüber-*

tragung, Wien, Pest, Leipzig, 1884; Helm, *Die Lehre von der Energie*, Leipzig, 1887; Wronsky, *Das Intensitätsgesetz*, Frankfurt a. O., 1888; Mach, "Geschichte und Kritik des Carnot'schen Wärmegesetzes" (*Sitzb. der Wien. Akad.*, 1892); and *Wärmelehre*. As regards pp. 85–86 in particular, such considerations were made mention of, first after Carnot, by Zeuner, *Grundzüge der mechanischen Wärmetheorie*, Leipzig, 2. Aufl., 1866. In the text of p. 86 the double resolution $MV^2/2$, $MV \cdot V/2$ is held to be possible, and, on this account, I have retained the general expression velocity instead of the square of the velocity as, still later, Ostwald did (*Berichte der kgl. sächs. Gesellschaft zu Leipzig*, Bd. XLIV, 1892, pp. 217–218). But I soon recognized that the potential-level is a scalar $V^2/2$ and cannot be a vector V or $V/2$. I did not speak of this further, since Popper had given a sufficient exposition of the correspondence between masses and quantities. This was also done by Friedrich Wolfgang Adler, "Bemerkungen über die Metaphysik in der Ostwald'schen Energetik" (*Vierteljahrsschr. für wiss. Philosophie und Soziologie*, Jahrg. 29, 1905, pp. 287–333).

To pp. 51–53.—Spaces of many dimensions seem to me not so essential for physics. I would only uphold them if things of thought like atoms are maintained to be indispensable, and if, then, also the freedom of working hypotheses is upheld.

To pp. 55 and 88.—The principle of the economy of thought is developed in detail in my later writings.

To p. 57.—I have repeatedly expressed the thought that the foundation of physics may be thermal or elec-

tric, in my *Mechanics* and *Analysis of the Sensations*. This thought seems to be becoming an actuality.

To pp. 60–64 and 88–90.—Space and time are not here conceived as independent entities, but as forms of the dependence of the phenomena on one another. I subscribe, then, to the principle of relativity, which is also firmly upheld in my *Mechanics* and *Wärmelehre*. Cf. "Zeit und Raum physikalisch betrachtet," in *Erkenntnis und Irrtum*, Leipzig, 1905 [(2d ed., 1906), pp. 434–448]; H. Minkowski, *Raum und Zeit*, Leipzig, 1909.

To p. 91.—The general remarks indicate the sensationalistic standpoint which I attained by studies in the physiology of the senses. Further developments in my *Bewegungsempfindungen* of 1875, *Analysis of the Sensations*, and *Erkenntnis und Irrtum*. I have also clearly shown there that the nervous, subjectivistic apprehensions which many physicists have for the physics of the inhabitants of Mars are quite groundless.

TRANSLATOR'S NOTES

To p. 15.—On the influence which Kant's *Prolegomena* exerted on Mach when a boy of fifteen, see note on p. 23 of *Analysis of the Sensations*, 1897.

To p. 17.—The investigators referred to on this page are not Kirchhoff and Helmholtz, whose works appeared at a later date (cf. *Mechanics*, p. x). Yet Kirchhoff is still regarded by many as the pioneer of descriptive physics. Cf. Mach's lecture "On the Principle of Comparison in Physics" in *Popular Scientific Lectures* (1898), pp. 236–258.

To p. 21.—On Stevinus's work, see, further, *Mechanics*, pp. 24–35, 49–51, 88–90, 500–501, 515–517; on Galileo's discussions of the laws of falling bodies, *ibid.*, pp. 128–155, 162–163, 247–250, 520–527, 563–567, and Ostwald's *Klassiker der exakten Wissenschaften*, Nr. 24, pp. 18–20, 57–59; on Huygens's researches on the centre of oscillation, *Mechanics*, pp. 173–186; on d'Alembert's principle, *ibid.*, pp. 331–343; on the principle of *vis viva*, *ibid.*, pp. 343–350; on Torricelli's theorem, *ibid.*, pp. 402–403; and, on the principle of virtual velocities, *ibid.*, pp. 49–77; A. Voss in his article, "Die Prinzipien der rationellen Mechanik," *Encykl. der math. Wiss.*, IV, 1 (1901), pp. 66–76; and, for a historical and critical review of the various proofs of the principle, R. Lindt, "Das Prinzip

der virtuellen Geschwindigkeiten," *Abhdl. zur Gesch. der Math.*, Bd. XVIII, 1904, pp. 147–196.[9]

To p. 34.—See the reprint of Gauss's paper in Ostwald's *Klassiker*, Nr. 167; especially p. 28.

Gauss's principle is discussed in Mach's *Mechanics*, pp. 350–364; Voss's above article in the *Encykl. der math. Wiss.*, pp. 84–87; and in the notes (by myself) to Nr. 167 of Ostwald's *Klassiker*, pp. 46–48, 59–68.

To p. 35.—From the *Wärmelehre:* On Carnot's principle and its developments, pp. 211–237; on the principle of Mayer and Joule, pp. 238–268; and on the uniting of the principles, by W. Thomson and Clausius, in particular, pp. 269–301.

An account of the development, meaning, and so on, of the principle of energy, which is, in essentials, the same as that in the *Popular Scientific Lectures* (3d ed., Chicago, 1898, pp. 137–185), is given in *Wärmelehre*, pp. 315–346. Cf. also the end of the note to pp. 51, 94, below.

To pp. 51, 94.—On many dimensional spaces as mathematical helps, cf. *Mechanics*, pp. 493–494.

In H. Weber's edition of Riemann's *Partielle Differential-Gleichungen*,[10] use was made of the idea of a particle in a space of n dimensions to represent what Hertz called "the position of a system" in ordinary

[9] Also separately as an Inaugural Dissertation. Cf. E. Lampe, *Jahrb über die Fortschr. der Math.*, 1904, pp. 691–692.

[10] *Die partiellen Differential-Gleichungen der mathematischen Physik. Nach Riemann's Vorlesungen in vierter Auflage neu bearbeitet von Heinrich Weber.* Two vols., Braunschweig, 1900–1901. The passage referred to occurs in the second part of the first volume.

space; the "position of a system" being the totality of the positions of the points of the system.

To p. 56.—On impact and other theories of gravitation, see J. B. Stallo, *The Concepts and Theories of Modern Physics*, 4th ed., London, 1900, pp. 52–65, v–vi, vii, xxi–xxiv (the three last references are to the "Preface to the Second Edition," which is not contained in the German translation by Hans Kleinpeter, published at Leipzig in 1901 under the title: *Die Begriffe und Theorieen der modernen Physik*, although this translation was made from the third English edition. This is the more regrettable as the preface referred to contains some indications of great value of Stallo's view—which closely resembled that of Mach—of the various forms of the law of causality; cf. below).

To pp. 60, 69, 73.—Clerk Maxwell's (*Matter and Motion*, London, edition of 1908, pp. 20–21) "General Maxim of Physical Science" is similar to Fechner's law of causality. It runs: "The difference between one event and another does not depend on the mere difference of the times or the places at which they occur, but only on differences in the nature, configuration, or motion of the bodies concerned."

The question as to the meaning of "causality" in dynamics is discussed in Bertrand Russell's work on *The Principles of Mathematics*, Vol. I, Cambridge, 1903, pp. 474–481.[11] On p. 478 is the sentence: "Causality, generally, is the principle in virtue of which, from a sufficient number of events at a sufficient number of

[11] Newton's laws of motion are discussed on pp. 482–488.

moments, one or more events at one or more new moments can be inferred."

The various forms of the law of causality were briefly described by J. B. Stallo, *op. cit.*, pp. xxxvi–xli[12]—a discussion not, unfortunately, translated in the German edition.

The present writer ("On Some Points in the Foundation of Mathematical Physics," *Monist*, Vol. XVIII, pp. 217–226, April, 1908) has attempted to formulate Mach's principle of causality and some other principles of physics in the exact mathematical manner to which we have become accustomed by the modern theory of aggregates, and to suggest some new problems in this order of inquiries.[13] It is my belief that this investigation is the only way in which we can become sure that the image of reality at which we aim, by successive approximations, is logically permissible; and also that only in this way can we succeed in formulating exactly the epistemological questions at the basis of physical science, and in answering them.[14] I will here give two illustrations of this.

The postulate as to the "intelligibility of nature," or the existence of a "process of reason in nature" may, it seems to me,[15] be further explained as follows. In our

[12] Cf. Stallo, *op. cit.*, pp. 25–26.

[13] Some of the conceptions and results applied here are contained in my article "On the General Theory of Functions," *Journ. für Math.*, Bd. CXXVIII, 1905, pp. 169–210.

[14] Cf. my article on "The Relevance of Mathematics," *Nature*, May 27, 1909, Vol. LXXX, pp. 282–384.

[15] However, Mr. Russell, who is probably right, tells me that, in his opinion, philosophers mean by this postulate "something much more general and vague."

scientific descriptions, we express elements (in Mach's sense; see the next note, to p. 61) as functions of other elements, determine by observation the character of these functions—whether they are, or may conveniently be considered, continuous, analytic, or so forth—and then deduce purely logically the image of the course of events, that is provided by this mathematical thought-model of nature. Thus, if a function of time, $f(t)$, is analytic, and we know its values for any small period $t_0 \ldots t_1$, we can deduce, in a purely logical fashion, by means of Taylor's theorem, its value for any other value of t whatever. We could not do this if that aspect of nature with which we deal here were not susceptible of this *mimicry by logic*, so to speak; and this is what we mean when we speak of the existence of science implying a conformity of nature to our reason.

In the second place, I will attempt an explanation of the attribute "uniformity" of nature. The difficulty lies in discovering the value of the maxim that like events result from the recurrence of like conditions, if like conditions never *do* recur. The solution seems to me to be as follows: Like conditions probably never do recur in the world around us, but we have learned by experience that we can imitate very closely the course of nature (in certain particulars) by means of a purely mathematical construction or *model*. In *this* model we can, of course, reproduce exactly similar circumstances as often as we wish. The above law applies literally to our model; and that the so-conditioned events in the model approximately coincide with the observed events of nature is, I take it, what we mean

when we say that nature is uniform. The point at issue here is quite similar to that discussed, *à propos* of Newton's rotating bucket, by Mach and Ward on the one side and Russell on the other (see my article, quoted above, in the *Monist*, p. 221).

Further references as to the meaning of causality in the light of modern theory of knowledge, and to the views of Mach, Stallo, and others, are as follows:

On the history of Mach's views on mass and on the substitution of the concept of function for that of causation, see *Mechanics*, pp. 555–556. The result of Mach's views which is of the greatest philosophical importance seems to be his disclosure of the character of the mechanical theory of nature (cf. the above translation, and *Mechanics*, pp. 495–501). This theory has been discussed at length and refuted—in many points after Mach's ideas—by James Ward, in the first volume of his *Naturalism and Agnosticism* (2d ed., London, 1903, 2 vols.).

Stallo (*op. cit.*, pp. 68–83) gave a sketch of the evolution of the doctrine of the conservation of energy and expressed views related to those of Mach. Thus, he said (*ibid.*, pp. 68–69): "In a general sense, this doctrine is coeval with the dawn of human intelligence. It is nothing more than an application of the simple principle that nothing can come from or to nothing"; and, in the preface to the second edition, he said (*ibid.*, pp. xl–xli): "But physicists, and especially mathematicians, are puzzled by the circumstance that not only has the law of causality always been applied before any experiential induction was thought of."

A few remarks by Poincaré on the principle of the conservation of energy on pp. 153–154 and 158–159 of his book *La science et l'hypothèse* (Paris, 6th ed.) are of an epistemological nature.

Cf. also Hans Kleinpeter, "Ueber Ernst Mach's und Heinrich Hertz' principelle Auffassung der Physik," *Archiv für systematische Philos.*, V, 1899, Heft 2; and "J. B. Stallo als Erkenntnisskritiker," *Vierteljahrsschr. für wiss. Philos.*, XXV, 1901, Heft 3.

A short exposition of the view[16] of the "symbolical physicists"—that our thoughts stand to things in the same relation as models to the objects they represent—is given by Ludwig Boltzmann in his article "Models" in the new volumes of the *Encyclopaedia Britannica* (Vol. XXX, 1902, pp. 788–791).

To p. 61.—Mach, in the memoir translated above, used *Erscheinungen* (phenomena) for what he afterwards (*Contributions to the Analysis of the Sensations*, Chicago, 1897, pp. 5, 11, 18) called by the less metaphysical name of "elements," thereby avoiding a verbal trap into which so many philosophers have fallen (see my article, referred to above, in the *Monist*, pp. 218–219, n. 6).

To p. 64.—The principle of the unique determination of natural events by others has been developed by Joseph Petzoldt, starting from Mach's considerations of 1872. Petzoldt's first work was entitled *Maxima, Minima und Ökonomie*, was printed in the *Vierteljahrsschr. für wiss. Philos.*, XIV., 1890, pp. 206–239, 354–366, 417–442, and was also printed separately as a

[16] This view I call the *typonoetic* theory

dissertation (Altenburg, 1891). On p. 12 of the reprint, Petzoldt states that the principles of Euler, Hamilton, and Gauss[17] are merely analytical expressions for the fact of experience that natural events are *uniquely* determined: the essential point is not the minimum but this uniqueness (*Einzigartigkeit*). Petzoldt's view that the thorough determinateness of all occurrences is a presupposition of all science was set forth in his paper: "Das Gesetz der Eindeutigkeit," *Vierteljahrsschr. für wiss. Philos.*, Vol. XIX, 1895, pp. 146–203.

Cf. also Mach's references to Petzoldt in *Mechanics*, pp. 552, 558, 562–563, 571–572, 575–577, 580–581; cf. pp. 10, 502–504, and *Wärmelehre*, pp. 324–327, for Mach's use of the principle of uniqueness, and a note farther on for further details about the principle of economy.

Petzoldt's views of the thoroughgoing uniqueness (*eindeutige Bestimmtheit*) of events were explained in his *Einführung in die Philosophie der reinen Erfahrung*[18]

[17] For German translations of some of the chief memoirs on these principles, very full of historical notes and modern references (by the present writer), see Ostwald's *Klassiker*, Nr. 167

[18] Erster Band: *Die Bestimmtheit der Seele*, Leipzig, 1900. A critical notice of this volume was given by W. R. Boyce Gibson in *Mind*, N.S., IX, No. 35 (July, 1900), pp. 389–401. The sentences following, in the text, are quoted from this review, pp. 391–392.

Petzoldt maintained: (1) that the facts upon which the time-worn principle of causation is founded do not justify us in admitting more or less than the unideterminateness of all that happens; (2) that the psychical states being non-unideterminable by each other, the attempt to make them explain one another is scientifically unthinkable; (3) that the only way out of the difficulty is to accept the doctrine of psycho-physical parallelism in the sense of Avenarius. In the sequences of the mental life, there is neither continuity, singleness of direction, nor uniqueness.

—in the first part an interpretation of the philosophy of Avenarius:

Whenever there are a number of possible ways in which, say, the movement of a body would be directed, Petzoldt showed, by a number of examples, that that path is selected, as a matter of fact, which possesses the following three elements of unideterminateness: (1) singleness of direction, (2) uniqueness, (3) continuity; for in satisfying these three conditions all indeterminateness is taken from its changes. The meaning of the first determining element is simply this, that as a matter of fact there is no *actual* ambiguity as to the sense in which any change takes place. Warm bodies left to themselves always grow cooler; heavy bodies left to themselves always fall downwards, not upwards. A first conceivable ambiguity is thus put to rest by Nature herself. In the second place Nature takes care that bodies shall move in such a way relatively to their *Bestimmungsmittel* or media of determination that the actual direction of motion differentiates itself from all the others by its uniqueness. It is only this uniqueness that gives to the actual change its right to be actualized, its right to be chosen in preference to any other possible change. Thus a ball moving freely on a horizontal plane passes from A in a rectilinear direction to B and on to C. It might conceivably have passed from B to D, where $B\,D$ is not collinear with $A\,B$; but though this course is a thinkable one it is not realized, because its realization would involve an ambiguity, for no reason could then be given why the direction of $B\,D$ was chosen in preference to the symmetrical direction $B\,E$. The direction $B\,C$ is in this case the only one that is unique and therefore unambiguous. The third element, that of continuity, secures the possibility of exact quantitative determination.

For every occurrence [says Petzoldt[19]] means of determination can be discovered whereby the occurrence is unambiguously determined, in this sense, that for every deviation from it, supposed to be brought about through the same means, at least one

[19] *Op. cit.*, p. 39.

other could be found which being determined in the same way would be its precise equivalent, and have as it were precisely the same right to be actualized.

By "means of determination" are meant just those means—e.g., masses, velocities, temperatures, distances—by the help of which we are able to grasp an occurrence as singled out by its uniqueness from a number of equally thinkable occurrences. The unideterminateness of things is both a fact of Nature and the *a priori* logical condition of there being a cosmos at all instead of a chaos. Our thought demands it from Nature, and Nature invariably justifies the demand. In this one supreme fact of the unideterminateness of all things the mind finds its rest. It is an ultimate fact, and one can no longer ask Why? when one comes to ultimate facts.

To p. 65.—On Archimedes's deduction of the law of the lever, and on the *uniqueness* of determination of equilibrium, see *Mechanics*, pp. 8–11, 13–14, 18–19.

To p. 66.—On the very similar methods employed by Galileo, Huygens, and Lagrange to demonstrate the law of equilibrium of the lever, see *Mechanics*, pp. 11–18.

To p. 76.—On Neumann's essay of 1870, cf. Mach's *Mechanics*, pp. 567–568, 572; Stallo, *op cit.*, pp. 196–200; Russell, *op. cit.*, pp. 490–491; the following note to p. 80; and C. Neumann, "Ueber die sogenannte absolute Bewegung" (*Festschrift, Ludwig Boltzmann gewidmet* , Leipzig, 1904, pp. 252–259).

To p. 80.—On relativity of position and motion, see Stallo, *op. cit.*, pp. 133–138, 183–206; Mach, *Mechanics*

pp. 222–238, 542–547, 567–573, and *Mechanik* (5. Aufl., 1904), pp. 257–263; James Ward, *op. cit.*, Vol. I, pp. 70–80; Russell, *op. cit.*, pp. 489–493; and my article in the *Monist*, quoted above, p. 221.

Planck has determined the form of the fundamental equations of mechanics which must take the place of the ordinary Newtonian equations of motion of a free mass-point if the principle of relativity is to be generally valid, in his paper: "Das Prinzip der Relativität und die Grundgleichungen der Mechanik" (*Verh. der Deutschen Phys. Ges.*, Vol. VIII, 1906, pp. 136–141).

To p. 80.—As regards Mach's definition of mass, it is interesting to find that Barré de Saint-Venant, in the paper[20] in which he announced and applied his independent discovery of Hermann Grassmann's[21] "outer multiplication," expressly drew attention to the use of "geometrical quantities" in treating mechanics by only letting space and time combinations enter, and not speaking of "forces." In the definition he gives of mass as a constant for each body, so chosen as to satisfy his "second law of mechanics":

$$mF_{mm} + m'F_{m'm} = 0,$$

he is exactly of the same view as Mach.

Cf. also H. Padé ("Barré de Saint-Venant et les

[20] "Mémoire sur les sommes et les différences géométriques, et sur leur usage pour simplifier la mécanique," *Compt. Rend.*, T. XXI, 1845, pp. 620–625. Cf. Hermann Hankel, *Vorlesungen über die complexen Zahlen und ihre Funcionen* (I. Theil, "Theorie der complexen Zahlensysteme"), Leipzig, 1867, p. 140.

[21] *Ausdehnungslehre von 1844.* For some account of the use of the methods of Hamilton and Grassmann in questions of mechanics, see *Mechanics*, pp. 527–528, 577–579.

principes de la mécanique," *Rev. générale des sciences*, XV, 1904, pp. 761–767), who points out that, in various points, de Saint-Venant's views coincide with those of Boltzmann.

Mach's definition has been accepted by most modern writers of books on dynamics; for example, Gian Antonio Maggi, *Principii della teoria matematica del movimento dei corpi*, Milano, 1896, p. 150; A. E. H. Love, *Theoretical Mechanics*, Cambridge, 1897, p. 87; Ludwig Boltzmann, *Vorlesungen über die Principien der Mechanik*, I. Theil, Leipzig, 1897, p. 22, and Poincaré—who, however, makes no mention of Mach's name—*La science et l'hypothèse*, 6th thousand, Paris, p. 123.

On criticisms of Mach's definition of mass, see *Mechanics*, pp. 539–540, 558–560.

To pp. 85–86, 93–94.—This analogy between heat and work done by gravity is known as "Zeuner's analogy," after Zeuner's remark in the second edition (1866) of his *Grundzüge der mechanischen Wärmetheorie*. See Georg Helm, *Die Energetik nach ihrer geschichtlichen Entwickelung*, Leipzig, 1898, pp. 254–266.

On the subject of a "comparative physics"—that is to say, a concise expression of extensive groups of physical facts, which is based on the analogies observed between the conceptions in different branches of physics —see Mach, *Mechanics*, pp. 496–498, 583; *Wärmelehre*, pp. 117–119; and *Pop. Sci. Lect.* (1898), p. 250;[22] L.

[22] Cf. also Mach, "Die Ähnlichkeit und die Analogie als Leitmotiv der Forschung" (*Annalen der Naturphilosophie*, Bd I, and *Erkenntnis und Irrtum*, 1906, pp. 220–231).

Boltzmann, in notes to his translation of Maxwell's paper of 1855 and 1856 "On Faraday's Lines of Force" (Ostwald's *Klassiker*, Nr. 69, pp. 100–102); M. Pétrovitch, *La mécanique des phénomènes fondée sur les analogies*, Paris, 1906; and Helm, *op. cit.*, pp. 253–266, 322–366.

It seems to me that the methods of a comparative physics, especially when aided by a calculus so well adapted to dealing with physical conceptions as that of Grassmann, Hamilton, and others, would afford a powerful means of discovering the ultimate principles of physics. Cf. the paper by de Saint-Venant referred to in the preceding note; M. O'Brien's paper "On Symbolic Forms Derived from the Conception of the Translation of a Directed Magnitude," in *Phil. Trans.*, Vol. CXLII, 1851, pp. 161–206; papers by Grassmann on mechanics[23] in his *Ges. Werke*, Bd. II, 2. Teil; and Maxwell, "On the Mathematical Classification of Physical Quantities," *Scientific Papers*, Vol. II, pp. 257–266. Cf. also Maxwell, *A Treatise on Electricity and Magnetism*, Oxford, 1873, Vol. I, pp. 8–29 (on the application of Lagrange's dynamical equations to electrical phenomena, see Vol. II, pp. 184–194); W. K. Clifford, *Elements of Dynamic*, Part I, "Kinematic," London, 1878; Hankel, *op. cit.*, pp. 114, 118, 126, 129, 132, 133, 134, 135, 137, 140; and Grassmann's *Ausdehnungslehre von 1844, passim.*

In this connexion, we may also give the following references: On the principle of energy, cf. Voss, *op.*

[23] Especially important is Grassmann's paper: "Die Mechanik und die Principien der Ausdehnungslehre," in *Math. Ann.*, XII 1877.

cit., pp. 104–107; on the Virial and the second law of thermodynamics, *ibid.*, pp. 107–109, and *Wärmelehre*, p. 364; on the localization of energy, Voss, *op. cit.*, pp. 109–115; on the treatment of mechanics by energetics, *ibid.*, 115–116, Mach, *Mechanics*, p. 585, and *Mechanik* (5. Aufl., 1904), pp. 405–406, Max Planck, *Das Prinzip der Erhaltung der Energie*, 2 Aufl., Leipzig and Berlin, pp. 166–213, Helm, *op. cit.*, pp. 205–252.

To p. 88.—A very slight indication of the principle of the economy of thought was, as Boltzmann[24] has remarked, contained in Maxwell's (1855) observation that, in order further to develop the theory of electricity, we must first of all simplify the results of earlier investigations and bring them into a form readily accessible to our understanding.

On the principle of the economy of thought in various branches of science, see Mach, *Mechanics*, pp. x–xi, 6, 481–494, 549, 579–583; *Wärmelehre*, pp. 391–395; *Pop. Sci. Lect.* (1898), pp. 186–213; A. N. Whitehead, *A Treatise on Universal Algebra*, Vol. I, Cambridge, 1898, p. 4; and my above-mentioned article in *Nature*, p. 383.

On Mach's formal principles of economy, simplicity, continuity, and analogy, see Voss, *op. cit.*, p. 20.

[24] Ostwald's *Klassiker*, Nr. 69, p. 100. The whole of the introduction to this paper of Maxwell's is of the greatest epistemological interest, as it states much more clearly than in any other of his writings what has been called the "symbolic" point of view in physics (see *ibid.*, pp. 3–9, 99–102.)

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